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CONTENTS

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WILLIAM JAMES

THOSE who had known William James only through his writings must have felt no little surprise to learn that he had all but reached his sixty-ninth birthday, and that he had for many years been made painfully aware of the organic trouble that finally took him from us. For during these later years of his life his most telling writings had appeared in rapid succession; writings so full of the spirit and vigor of youth that it was difficult even for his friends to realize that he was approaching the limit of three score years and ten, and that infirmity threatened him.

These later years, as all his readers know, were devoted to the promulgation of certain metaphysical doctrines, and it is indicative of the persuasive power of the man that the audience gained by him among men of science in the beginning of his career was not lost when he asked them to consider subjects usually looked upon as quite foreign to their mode of thought.

For it must be remembered that he made his first impression as a man of marked ability among scientific men. He was educated in the Lawrence Scientific School. He accompanied Agassiz on one of his scientific expeditions. He took the degree of doctor of medicine at the Harvard School, and shortly after devoted some years to the teaching of physiology. And it was in connection with physiological studies that we first have indications of a fully awakened interest in the nature of the mental changes that accompany bodily activities. In his early psychological essays, such for instance as those on instinct and habit, and in his later

emotional theory, we see the predominant influence of his physiological studies; and his insistence upon the foundation of a psycho-physical laboratory in connection with his university tells the same story.

He had, to be sure, no mean artistic endowment, as was indicated by the promise of his youthful work as a student under masters of painting; and fully demonstrated in the inimitable literary quality that made his writings in diverse fields so fascinating, and in the fertility of imagination displayed in every piece of work he undertook. Nevertheless, it is easy to see that, had the circumstances of his early years been but slightly different, he might very well have devoted his life to pure science alone.

Even when he finally turned his energies to the study of the fundamental problems upon which all science must in the end be based, he held his hearers and readers, not only by his matchless mode of expression, nor only because of the keenness of his criticism and the value of his teachings, but also in large part because his utterances were appreciated to be those of a man who exemplified to the full the attitude of the faithful devotee of science.

In fact, as I think of his work as a whole, I am prepared to believe that his readers of the future will find his most striking traits to have been the very ones that men of science hold as their ideals; viz., an intense interest in investigation in all matters to which his attention was turned; and an equally intense devotion to the search for truth, with which was joined an unwillingness to treat lightly any data whatever that might possibly be found to be significant.

And yet it has become apparent to us today that he was first and foremost a psychologist. And to service in that field he devoted all of the artistic gift that was his, and all the powers that he had gained in the study of the more rigid sciences: and had he ceased writing twenty years ago, when his masterly "Principles of Psychology" appeared, he would perhaps have been known only as James the psychologist.

That he was the ablest and most influential psychologist of our time can not be questioned; and I am inclined to agree with Professor Dewey that men of future generations may look upon him as the greatest psychologist that has ever lived.

His work, it is true, was not, strictly speaking, systematic. As he wrote to me in one of his familiar letters, he always found it necessary "to overcome a certain primary repugnance for everything put in abstract and schematic shape." But this, after all, was natural to a man of his temper. For the system-maker, dealing as he does with broad generalizations, must inevitably fail to cover by his formulations many details that are imperfectly comprehended; and must be constantly tempted to pass these by as less significant than they really are. For James these very obdurate details had especial interest. He delighted to lay them bare even though they could not be systematized: and being the soul of candor, he found himself utterly incapable of hiding from his fellows any insight that he had gained through his exceptional powers of analysis, which often brought to light a multiplicity of interesting elements in what had too commonly been assumed to be unanalyzable. These acute analyses, so constantly illustrated by reference to concrete instances, have furnished to the psychologist of the future the richest of data, a veritable mine of wealth for the scheme maker of a later day.

During the last years of his life, however, James, in his published writings, dealt especially with subjects philosophical rather than psychological. But even in this realm which the man of science hesi-

tates to enter, we still feel the influence of his scientific predilections in the emphasis he gave to a "radical empiricism," and in his vigorous attacks upon the monistic philosophers whom he accused of blinding themselves to the vast variety within experience in their efforts to find unity in what he believed to be really a "pluralistic universe."

In this philosophical field his work was on the whole less constructive than critical. But his criticism was so cogent, and was driven home with so much power, that even those who were altogether out of sympathy with his general philosophical position were compelled to listen with attention and respect.

His name has of late come to be inseparably connected with the philosophical tenet known as pragmatism. Although it is true that the first formulation of this doctrine was due to Charles Peirce, as James himself took every opportunity to explain; and equally true that its acceptance has been implicit in the writings of many of our philosophical fathers, as James also acknowledged in calling it "a new name for some old ways of thinking": nevertheless, it must be acknowledged that it is due mainly to his brilliant expositions, that the importance of the doctrine has become so evident that it can never again be overlooked as it has been in the past.

The full significance of this doctrine can not yet be fairly estimated. James himself was ready to acknowledge this; and his latest book "The Meaning of Truth" contains many indications that, as his thought developed, he was gaining new light in regard to the implications of the postulate. Whatever it may in the end have to say of "the eternal verities," it at least teaches us that we accept a conception as true just so long as it is "workable"; that our conceptions of truth are relative to the uses to which these conceptions are put; a fact which surely implies that doubt indicates no more than our discovery that these conceptions are not thoroughly "workable," and that modifications of them which shall be closer to reality are required if they are to maintain their full value in our lives.

This teaching, if once firmly grasped, is seen to have bearings that reach quite beyond the realm of theoretical metaphysics, having special importance in the field of concrete ethics; and I am convinced that, had his life been spared, James would surely have laid stress upon the ethical implications of the doctrine he so persistently pressed upon us. For no one could come to know James even casually without feeling that he was in the presence of a man of unusual moral force. In fact, no one can read far in his pages without gaining this same impression, which was especially exemplified in such books as his "Will to Believe and other Essays on Popular Philosophy," and his "Talks to Teachers on Psychology and to Students on Some of Life's Ideals." He never posed as an ethical teacher, to be sure, nor was he given to sermonizing; yet his readers were uplifted as they breathed, as it were, the moral atmosphere in which his thought moved.

It is not surprising then that a man of this nature should have taken a very deep interest in the phenomena of religious life, which yielded that remarkable series of Gifford lectures later published under the title of "Varieties of Religious Experience." Had James written no other work, his life would have been a notably efficient one. In this book he gathered together the results of the investigations of others, and of his own keen observations; and placed these results before his readers in a form so persuasive, and withal so reverent, that while, on the one hand, he aroused no hos-

tility among the dogmatists, on the other, he led many men who had been driven from the churches to realize the deep significance of religion in their own lives.

Perhaps no other one of his books shows so fully the character of the man as he was known to his friends; and after all it was his character, as divulged in his writings, that made his life so influential. He was a true representative of that spirit of liberty which led to the foundation of the New England Colony, and which has been so potent in shaping the destinies of his native country. He rejoiced that he and his fellows did not have to meet the pressure of the social order which so hampered the lives of those born on foreign soils. He was the strongest of individualists, firm in his belief in the right of the individual to develop himself in the manner of his own choosing; a belief which found expression on the philosophical side in his "Pluralistic Universe," and on the practical side in his opposition to the imperialistic tendencies developed in this country in connection with our acquisition of the Philippine Islands.

And it was this general attitude of mind that yielded that depth of sympathy with all sorts and conditions of men which was so fully exemplified in his "Varieties of Religious Experience" above referred to.

Here was a man upon whom had been showered the highest of honors by societies of learned men the world over; who nevertheless remained as modest as a child; ever eager to learn from the humblest human soul the secrets of its innermost nature; and ever ready to acknowledge the limitations of his own insight. Referring to a little discussion between Schiller and myself a few years ago, he wrote to me, "I don't fully understand Schiller's position, or yours;—or my own, *yet.*" How could we think of him as anything but young, who maintained to the end such open-mindedness, such mental plasticity. How could we fail to honor a man who displayed such intellectual integrity.

I can not close this inadequate survey of the life work of my beloved friend and master without a word of personal tribute which I doubt not will find an echo in the thought of very many others. But for his interest in some crude work of mine in my youth, and before we had ever met, I should probably never have discovered that I had in me the capacity to think or write anything that might be worthy of the attention of any psychologist. To this one kindly act I can trace the development of a side of my nature which has given life for me a special interest it could not otherwise have had. And during the years that followed he never missed an opportunity to write a word of encouragement whenever any work of mine appeared to him to have a shred of value. What he did for me he doubtless did for many another. We have lost an inspiring master. But more than that we have lost an ever faithful and beloved friend.

Henry Rutgers Marshall

SURFACE TENSION IN RELATION TO CELLULAR PROCESSES. II

The first to suggest that surface tension is a factor in muscular contraction was D'Arsonval, but it was Imbert who, in 1897, directly applied the principle in explanation of the contractility of smooth and striated muscle fiber. In his view the primary conditions are different in the former from what obtain in the latter. In smooth muscle fiber the extension is determined, not by any force inside it, but by external force such as may distend the organ (intestine, bladder and arteries) in whose wall it is found. The "stimulus" which causes the contraction increases the

surface tension between the surface of the fiber and the surrounding fluid, and this of itself has the effect of making the fiber tend to become more spherical or shorter and thicker, which change in shape does occur during contraction. He did not, however, explain how the excitation altered the surface tension, except to say that its effect on surface tension is like that of electricity, with which the nerve impulse presents some analogy. In striated fiber, on the other hand, the discs constituting the light and dim bands have each a longitudinal diameter which is an effect of its surface tension, and this causes extension of the fiber during rest. When a nerve impulse reaches the fiber the surface tension of the discs is altered and there results a deformation of each involving a shortening of its longitudinal axis and thus a shortening of the whole fiber.

According to Bernstein, in both smooth and striated muscle fiber there is, in addition to surface tension, an elastic force residing in the material composing the fiber which, according to the conditions, sometimes opposes and sometimes assists the surface tension. The result is that in the muscle fiber at rest the surface must exceed somewhat that of the fiber in contraction. In both conditions the sum of the two forces, surface tension and elasticity, must be zero. In contraction the surface tension increases and with it the elasticity also. Taken as a whole, this would not explain the large force generated in contraction, for the energy liberated would be the product of the surface tension and the amount representing the diminution of the surface due to the contraction. As the latter is very small the product is much below the amount of energy in the form of work done actually manifested. To get over this difficulty Bernstein postulates that in muscle fibers, whether smooth or striated, there are fibrils surrounded by sarcoplasma, and that each fibril is formed of a number of cylinders or biaxial ellipsoids singly disposed in the course of the fibril, but separated from each other by elastic material and surrounded by sarcoplasma. Between the ellipsoids and the sarcoplasma there is considerable surface tension which prevents mixture of the substances constituting both. The excitation through the nerve impulse causes an increase of surface tension in these ellipsoids, and they become more spherical. In consequence the decrease in surface of all the ellipsoids constituting a fibril is much greater than if the fibril were to be affected as an individual unit only by an increase of surface tension, and thus the surface energy developed would be correspondingly greater. The ellipsoids, Bernstein explains, are not to be confused with the discs, singly and doubly refractive in striated fiber; for these, he holds, are not concerned in the generation of the contraction, but with the processes that make for rapidity of contraction. The extension of a muscle after contraction is due to the elastic reaction of the substance between the ellipsoids in the fibrils. Bernstein further holds that fibrils of this character occur in the protoplasm of *Amœbæ,* in the stalk of *Vorticella* and in the ectoplasma of *Stentor,* and this explains their contractility.

It may be said in criticism of Bernstein's view that his ellipsoids are from their very nature non-demonstrable structures and, therefore, must always remain as postulated elements only. Further, it may be pointed out that he attributes too small a part to surface tension in the lengthening of the fiber after contraction, and that the elasticity which muscle appears to possess is, in the last analysis, but a result of its surface tension.

As regards Quincke's explanation of pro-

toplasmic movement and streaming, as well as of muscular contraction, Bütschli has shown that it is based on a mistaken view of the structure of the cell in *Chara* and other plant forms in which protoplasmic streaming occurs. Bütschli's own hypothesis, however, is defective in that it postulates a current in the fluid medium just outside the *Amœba* and backward over its surface, the existence of which Berthold denies, and Bütschli himself has been unable to demonstrate, even with the aid of fine carmine powder in the fluid. He did, indeed, observe a streaming in the water about a creeping *Pelomyxa,* but the current was in the opposite direction to that demanded by his hypothesis. Further, his failure to demonstrate the occurrence of the postulated back-flow in the water about the contracting or moving mass of an *Amœba* or a *Pelomyxa,* makes it difficult to accept the hypothesis he advanced to explain that back-flow, namely, that rupture of peripheral vesicles (*Waben*) of the protoplasm occurs with a consequent discharge of their contents (proteins, oils and soaps) into the surrounding fluid. Surface tension, further, on this hypothesis would be an uncertain and wasteful factor in the life of the cell. On *a priori* grounds also it would seem improbable that this force should be generated outside instead of inside the cell.

One common defect of all these views is that they made only a limited application of the principle of surface tension. This was because some of its phenomena were unknown and especially those illustrating the Gibbs-Thomson principle. With its aid and with the knowledge of the distribution of inorganic constituents in animal and vegetable cells that microchemistry gives us we can make a more extended application of surface tension as a factor in cellular life than was possible ten years ago.

In regard to muscle fiber this is particularly true, and microchemistry has been of considerable service here. From the analyses of the inorganic constituents of striated muscle in vertebrates made by J. Katz and others we know that potassium is extraordinarily abundant therein, ranging from three and a half in the dog to more than fourteen times in the pike the amount of sodium present. How the potassium salt is distributed in the fiber was unknown before 1904, in which year, by the use of a method, which I had discovered, of demonstrating the potassium microchemically, the element was found localized in the dim bands. Later and more extended observations suggested that in the dim band itself, when the muscle fiber is at rest, the potassium is not uniformly distributed, and it was found to be the case in the wing muscles of certain of the insecta—as, for example, the scavenger beetles—in which the bands are broad and conspicuous enough to permit ready observation on this score. In these the potassium salt was found to be localized in the zones of each dim band adjacent to each light band. Subsequently Miss M. L. Menten, working in my laboratory and using the same microchemical method, found the potassium similarly limited in its distribution in the muscle fibers of a number of other insects. She determined, also, that the chlorides and phosphates have a like distribution in these structures, and it is consequently probable that sodium, calcium and magnesium have the same localization.

Macdonald has also made investigations on the distribution of potassium in the muscle fiber of the frog, crab and lobster, using for this purpose the hexanitrite reagent. He holds, as a result of his observations, that the element in the uncontracted fibril is limited to the sarcoplasm in the immediate neighborhood of the

singly refractive substance, while it is abundantly present in the central portion of each sarcomere of the contracted fibril—that is, in the doubly refractive material. I am not inclined to question the former point, as I have not investigated the microchemistry of the muscle in the crab and lobster, and my only criticism would be directed against placing too great reliance on the results obtained in the case of frog's muscle. The latter is only very slowly penetrated by the hexanitrite reagent, and, apparently because of this, alterations in the distribution of the salts occur and, as I have observed, the potassium may be limited to the dim bands of one part of the contracted fiber and may be found in the light bands of another part of the same. In the wing muscles of insects in the uncontracted condition such disconcerting results are not so readily obtained, owing, it would seem, to the readiness with which the fibrils may be isolated and the almost immediate penetration of them by the reagent. Here there is no doubt about the occurrence of the element in the zones of the dim band immediately adjacent to the light bands.

Whether the potassium in the resting fiber is in the sarcoplasm or in the sarcostyle I would hesitate to say. It may be as Macdonald claims, but I find it difficult to apply in microchemical studies of muscle fiber the concepts of its more minute structure gained from merely stained preparations. Because of this difficulty I have refrained from using here, as localizing designations, other expressions than "light bands" and "dim bands." The latter undoubtedly include some sarcoplasm, but in the case of the resting fiber I am certain only of the presence of potassium, as described, in the dim band regarded as an individual part, and not as a composite structure.

Now, on applying the Gibbs-Thomson principle enunciated above, this distribution would seem to indicate that in the dim band of a fibril the surface tension is greatest on its lateral walls, in consequence of which the potassium salts are concentrated in the vicinity of the remaining surfaces, *i. e.*, those limiting the light bands. This explanation would seem to be confirmed by the observations I made on the contracted fibrils of the wing muscles of a scavenger beetle. In these the potassium was found uniformly distributed throughout each dim band, which, instead of being cylindrical in shape as in the resting element, is provided with a convexly curved lateral wall, and therefore with a smaller surface than the mass of the dim band has when at rest. This contour suggests that the surface tension on the lateral wall is lessened to an amount below that of either terminal surface, followed by a redistribution of the potassium salt to restore the equilibrium thus disturbed. The consequent shortening of the dim bands of the fibrils would account for the contraction of the muscle.

How the surface tension of the lateral wall of the dim band is lessened in contraction is a question which can only be answered after much more is known of the nature of the nerve impulse as it reaches the muscle fibril, and of the part played by the energy set free in the combustion process in the dim bands. It may be that electrical polarization, as a result of the arrival of the nerve impulse, develops on the surface of the lateral wall, and as a consequence of which its surface tension is diminished. The energy so lost appears as work, and it is replaced by energy, one may suppose, derived from the combustion of the material in the dim band. In this case the disturbance of surface tension would be primary, while the combustion process would be secondary, in the order of time.

In support of this explanation may be cited the fact that the current of action in muscle precedes in time the contraction itself—that is, the electrical response of the stimulus occurs in the latent period and immediately before the contraction begins.

It may, however, be postulated, on the other hand, that the chemical changes occur in those parts of the dim band immediately adjacent to the light bands, and as a result the tension of the terminal surfaces may be increased, this resulting in the shortening of the longitudinal axis of the dim band and the displacement laterally of the contents. This would imply that the energy of muscle contraction comes primarily from that set free in the combustion process, and not indirectly as involved in the former explanation.

Whatever may be the cause of the alteration in surface tension, there would seem to be no question of the latter. The very alteration in shape of the dim band in contraction makes it imperative to believe that surface tension is concerned. The redistribution of the potassium which takes place as described in the contracting fibrils of the wing muscles of the scavenger beetle can be explained in no other way than through the alteration of surface tension.

In the smooth muscle fiber potassium is also present and in close association throughout with the membrane. When a fresh preparation of smooth muscle is treated so as to demonstrate the presence of potassium, the latter is shown in the form of a granular precipitate of hexanitrite of sodium, potassium and cobalt in the cement substance between the membranes of the fibers. In the smooth muscle fibers in the walls of the arteries in the frog the precipitate in the cement material is abundant, and its disposition suggests that it plays some part in the rôle of contraction. Inside of the membrane potassium occurs, but in very minute quantities, which, with the cobalt sulphide method, gives a just perceptible dark shade to the cytoplasm as a whole. Microchemical tests for the chlorides and phosphates indicate that the cytoplasm is almost wholly free from them, and consequently there is very little inorganic material inside of the fiber. Chlorides and phosphates, but more particularly the former, are abundant in the cement material, and their localization here would seem to indicate that the potassium of the same distribution is combined chiefly as chloride.

In smooth muscle fiber, then, the potassium is distributed very differently from what it is in striated fiber, and on first thought it seemed difficult to postulate that the contraction could be due to alterations of surface tension. This, however, would appear to be the most feasible explanation, for the potassium salts in the cement substance might be supposed to shift their position under the influence of electrical force so as to reach the interior of the membranes of the fibers, in which case the surface tension of the latter would be immediately increased and the fiber itself would in consequence at once begin to contract. The slowness with which this shifting into, or absorption by, the membrane of the potassium salts would take place would also account for the long latent period of contraction in smooth muscle.

It is of interest here to note that the potassium ions have the highest ionic mobility (transport number) of all the elements of the kationic class, except hydrogen, which are found to occur in connection with living matter. Its value in this respect is half again as great as that of sodium, one eighth greater than that of calcium and one seventh greater than that of magnesium. This high migration velocity of potassium ions would make the ele-

ment of special service in rapid changes of surface tension.

Loew has pointed out that potassium in the condensation processes of the synthesis of organic compounds has a catalytic value different from that of sodium. For example, ethyl aldehyde is condensed with potassium salts to aldol, with sodium salts to crotonic aldehyde (Kopf and Michael). Potassium is, but sodium is not, effective in the condensation of carbon monoxide. When phenol is fused with potassium salts condensation products like diphenol are produced, but when sodium salts are used the products are dioxybenzol and phloroglucin (Barth). It is, therefore, not improbable that potassium, along with those properties which come from its ionic mobility, has a special value in the metabolism of the dim bands of striated muscle fiber and in the condensation synthesis which characterizes the chromatophors of Protophyta (*Spirogyra, Zygnema*).

With the use of this method of determining differences in surface tension in cells it is possible, in some cases at least, to ascertain whether this force plays a part in both secretion and excretion, and evidence in favor of this view can be found in the pancreatic cells of the rabbit, guinea-pig, and in the renal cells of the frog. In the pancreatic cells there is an extraordinary condensation of potassium salts in the cytoplasm of each cell adjacent to the lumen of the tubule, and during all the phases of activity—except, it would appear, that of the so-called "resting-stage"—potassium salts occur in, and are wholly confined to, this part of each cell. It is difficult to say whether they pass into the lumen with the secretion and their place taken by more from the blood-stream and lymph, but the important point is that the condensation of potassium salts immediately adjacent to the lumen seems to indicate a lessened surface tension on the lumen surface of the cell.

According to Stoklasa[3] the pancreas of the pig is much richer in potassium than in sodium, the dried material containing 2.09 per cent. of potassium and 0.28 per cent. of sodium, while the values for the dried material of ox muscle are, as he determined them, 1.82 and 0.26 per cent., respectively. It is significant that in the pancreas this large amount of potassium should be localized as described.

In the renal cells of vertebrates there is usually a considerable amount of potassium salts distributed throughout the cytoplasm. These cells are always active in the elimination of the element from the blood, and it is in consequence not possible to determine whether there are differences in surface tension in them. Under certain conditions, however, these can be demonstrated. In the frogs which have been kept in the laboratory tanks throughout the winter, and in the blood of which the inorganic salts have been, because of the long period of inanition, reduced to almost hypotonic proportions, the renal cells are very largely free from potassium. When it is present it is usually diffused throughout the cytoplasm. If now a few cubic centimeters of a decinormal solution of potassium chloride be injected into the dorsal lymph sacs of one of these frogs, and after twenty minutes the animal is killed, appropriate treatment, with the cobalt reagent, of a thin section of the fresh kidney made by the carbon dioxide freezing method, reveals in the cells of certain of the tubules a condensation of potassium salts in the cytoplasm immediately adjacent to the wall of the lumen. There is also a very slight diffuse reaction throughout the remainder of the cytoplasm, except in that part immediately adjacent to the external boundary

[3] Stoklasa gave the values in K_2O and Na_2O.

of the tubule. In these cells the potassium injected into the lymph circulation is being excreted, and the condensation of the element at or near the surface of the lumen is evidence that there the tension is less than at the other extremity of the cell.

These facts are in their significance in line with some observations that I have made on the absorption of soluble salts by the intestinal mucosa in the guinea-pig. When the "peptonate" of iron was administered in the food of the animal it was not unusual to find that in the epithelial cells of the villi the iron salt was distributed through the cytoplasm, but its concentration, as a rule, was greatest in the cytoplasm adjacent to the inner surface of the cell, from which it diffused into the underlying tissue. Here also, inferentially, surface tension is lower than elsewhere in the cell.

It would perhaps be unwise to form final conclusions at this stage in the progress of the investigation of the subject, but the results so far gained tempt one to adopt as a working hypothesis *that in the secreting or the excreting cell lower surface tension exists at its secreting or excreting surface than at any other point on the cell surface.* How this low surface tension is caused or maintained it is impossible to say, but, whatever the solution of the question may be, it is important to note that we must postulate the participation of this force in renal excretion in order to explain the formation of urines of high concentration. These have a high osmotic pressure, as measured by the depression of the freezing-point, while the osmotic pressure of the blood plasma determined in the same way is low. On the principle of osmosis alone, as it is currently understood, this result is inexplicable, for the kinetic energy, as required in the gas theory of solutions, should not be greater, though it might be less, in the urine than in the blood. It is manifest that in the formation of concentrated urines energy is expended. We know also from the investigations of Barcroft and Brodie that the kidney during diuresis absorbs much more oxygen per gram weight than the body generally, and that, assuming it is used in the combustion of a proteid, a very large amount of energy is set free, very much more, indeed, than is necessary. It has also been observed that a portion of the energy set free is found in a higher temperature in the excretion than obtains in the blood itself circulating through the kidney. This large expenditure of energy is, probably, a result of the physiological adaptation of the principle of the "factor of safety," which, as Meltzer has pointed out, occurs in other organs of the body.

In cell and nuclear division surface tension operates as a force, the action of which can not be completely understood till we know more of the part played by the centrosomes and centrosphere. That this force takes part in cell reproduction has already been suggested by Brailsford Robertson. He has devised an ingenious experiment to illustrate its action. If a thread moistened with a solution of a base is laid across a drop of oil in which is dissolved some free fatty acid the drop divides along the line of the thread. When the latter is moistened with soap the drop divides in the same way and in the same plane. The soap formed in one case and present in the other, it is explained, lowers the surface tension in the equatorial plane of the drop, and this diminution results in streaming movement away from that plane which bring about the division. He suggests that in cell division there is a liberation of soaps in the plane of division which set up streaming movements from that plane to-

wards the poles and terminating in the division of the cytoplasm of the cell.

I have observed in the cells of *Zygnema* about to divide a remarkable condensation of potassium in the plane of division. In the "resting" cell of this *Alga* the potassium is, as a rule, more abundant in the cytoplasm near the transverse walls of the thread, and only traces of the element are to be found along the line of future division of the cell. But immediately after division has taken place the potassium is concentrated in the plane of division. This would seem to indicate that surface tension in the plane of division is, as postulated by the deduction from the Gibbs-Thomson principle, lower than it is on the longitudinal surface, and lower, especially, than it is on the previously formed transverse septa of the thread.

One must not, however, draw from this the conclusion that in all dividing cells surface tension is lower in the plane of division than it is elsewhere on the surface of the dividing structure. All that it means is that in the dividing cell of *Zygnema* the condition already exists along the plane of division, which subsequently makes for low surface tension in the cell membrane immediately adjacent to each transverse septum in the confervoid thread. If the evidence of low surface tension vanished immediately after division was complete, then it might be held that it determined the division. As it is, the low surface tension in this case is the result and not the cause of the division.

This conclusion is corroborated by the results of observations on the cells of the ovules of *Lilium* and *Tulipa*. The potassium salts in these are found condensed in minute masses throughout the cytoplasm. When division is about to begin the salts are shifted to the peripheral zone of the cytoplasm, and when the nuclear membrane disappears not a trace of potassium is now found in the neighborhood of the free chromosomes, a condition which continues till after nuclear division is complete. The absence of potassium, the most abundant basic element in the cytoplasm, would indicate that soaps are not present, and appropriate treatment of such cells, hardened in formaline only, with scarlet red demonstrates that fats, including lecithins, are absent also. This would seem to show that high instead of low surface tension prevails about the nucleus during division. During the "resting" condition of the nucleus this high tension is maintained, for, except in very rare cases, and these of doubtful character, there is no condensation of inorganic salts in the neighborhood or on the surface of the nuclear membrane. It is also to be noted that the nucleus, with exceptions, the majority of which are found in the protozoa, is of spherical shape, which also postulates that high surface tension obtains either in the cytoplasmic layer about the nucleus or in the nuclear membrane itself. It may also be suggested that high surface tension, and not the physical impermeability of the nuclear membrane, is the reason why the nucleus is, as I have often stated, wholly free from inorganic constituents.

It does not follow from all this that surface tension has nothing to do with cell division. If, as Brailsford Robertson holds, surface tension is lowered in the plane of division, then the internal streaming movement of the cytoplasm of each half of the cell should be towards that plane, and, in consequence, not separation, but fusion of the two halves, would result. The lipoids and soaps would indeed spread superficially on the two parts from the equatorial plane towards the two poles, and, according to the Gibbs-Thomson principle, they would not distribute themselves through the cytoplasm in the plane of division, except as a

result of the formation of a septum in that plane. In other words, the septum has first to exist in order to allow the soaps and lipoids to distribute themselves in a streaming movement over its two faces. In Brailsford Robertson's experiment this septum is provided in the thread. If, on the other hand, surface tension is higher about the nucleus in and immediately adjacent to the future plane of division, then constriction of the nucleus in that plane will take place accompanied or preceded by an internal streaming movement in each half towards its pole, and a consequent traction effect on the chromosomes which are thus removed from the equatorial plane. When nuclear division is complete, then a higher surface tension on the cell, itself limited to the plane of division, would bring about there a separation of the two halves, a consequent condensation on each side of that plane of the substances producing the low tension elsewhere, and thereby also the formation of the two membranes in that plane.

In support of this explanation of the action of surface tension as a factor in division I have endeavored to ascertain if, as a result of the Gibbs-Thomson principle, there is a condensation of potassium salts in the cytoplasm at the poles of a dividing cell, that is, where surface tension, according to my view, is low. The difficulty one meets here is that, in the higher plant forms, cells preparing to divide appear to be much less rich in potassium than those in the "resting" stage, and under this condition it is not easy to get unambiguous results, while in animal cells potassium may even in the resting cell be very minute in quantity, as, for example, in *Vorticella,* in which, apart from the contractile stalk, it is limited to one or two minute flecks in the cytoplasm. Instances of potassium-holding cells undergoing division are, however, found in the spermatogonia of higher vertebrates (rabbit, guinea-pig), and in these the potassium is gathered in the form of minute and thin caplike layers at each pole of the dividing cell.

This of itself would appear to show that surface tension is less in the neighborhood of the poles than at the equator of the dividing cell, but I am not inclined to regard the fact as conclusive, and a very large number of observations to that end must be made before certainty can be attained. I am, nevertheless, convinced that it is only in this way that we can finally determine whether differences of surface tension in dividing cells account, as I believe they do, for all the phenomena of cell division. The difficulties to be encountered in such an investigation are, as experience has shown me, much greater than are to be overcome in efforts to study surface tension in cells under other conditions, but I am in hopes that what I am now advancing will influence a number of workers to take up research in microchemistry along this line.

I must now discuss surface tension in nerve cells and nerve fibers. I have stated earlier in this address that I hold that the force concerned in the production of the nerve impulse by the nerve cell is surface tension. The very fact that in the repair of a divided nerve fiber the renewal of the peripheral portion of the axon occurs through a movement—a flowing outward, as it were—of the soft colloid material from the central portion of the divided fiber, is, in itself, a strong indication that surface tension is low here and high on the cell body itself. This fact does not stand alone. I pointed out six years ago that potassium salt is abundant along the course of the axon and apparently on its exterior surface, while it is present but in traces in the nerve cell itself. In the latter chlorides also are present only in traces, and therefore sodium, if present, is there in more

minute quantities, while haloid chlorine is abundant in the axon. Macdonald has also made observations as to the occurrence of potassium along the course of the axon, and has in the main confirmed mine. We differ only as to mode of the distribution of the element in the axon, and the manner in which it is held in the substance of the latter; but, whichever of the two views may be correct, it does not affect what I am now advancing. Extensive condensation or adsorption of potassium salts in or along the course of the axon, while the nerve cell itself is very largely free from them, can have but one explanation on the basis of the Gibbs-Thomson principle, and that explanation is that surface tension on the nerve cell itself must be high while it is low on or in its axon.

The conclusions that follow from this are not far to seek. We know that an electrical displacement or disturbance of ever so slight a character occurring at a point on the surface of a drop lowers correspondingly the surface tension at that point. What a nerve impulse fundamentally involves we are not certain, but we do know that it is always accompanied by, if not constituted of, a change of electrical potential, which is as rapidly transmitted as is the impulse. When this change of potential is transmitted along an axon through its synaptic terminals to another nerve cell, the surface tension of the latter must be lowered to a degree corresponding to the magnitude of the electrical disturbance produced, and, in consequence, a slight displacement of the potassium ions would occur at each point in succession along the course of its axon. This displacement of the ions as it proceeded would produce a change of electrical potential, and thus account for the current of action. The displacement of the ions in the axon would last as long as the alteration of surface tension which gave rise to it, and this would comprehend not more than a very minute fraction of a second. Consequently, many such variations in the surface tension of the body of the nerve cell would occur in a second; and, as the physical change concerned would involve only the very surface layer of the cell, a minimum of fatigue would result in the cell, while little or none would develop in the axon.

It may be pointed out that in medullated nerve fibers the lipoid-holding sheath, in close contact as it is with the axon, must of necessity maintain on the course of the latter a surface tension low as compared with that on the nerve cell itself, which, as the synaptic relations of other nerve cells with it postulate, is not closely invested with an enveloping membrane. In non-medullated nerve fibers the simple enveloping sheath may function in the same manner, and probably, if it is not rich in lipoid material, in a less marked degree.

What further is involved in all this, what other conclusions follow from these observations, I must leave unexplained. It suffices that I have indicated the main points of the subject, the philosophical significance of which will appear to those who will pursue it beyond the point where I leave it.

In bringing this address to a close I am well aware of the fact that my treatment of the subjects discussed has not been as adequate as their character would warrant. The position which I occupy imposes limits, and there enters also the personal factor to account in part for the failure to achieve the result at which I aimed. But there is, besides, the idea that in applying the laws of surface tension in the explanation of vital phenomena I am proceeding along a path into the unknown which has been as yet only in a most general way marked out by pioneer investigators, and in consequence, to avoid mistakes, I have been con-

strained to exercise caution, and to repress the desire to make larger ventures from the imperfectly beaten main road. Perhaps, after all, I may have fallen into error, and I must therefore be prepared to recall or to revise some of the views which I have advanced here, should they ultimately be found wanting. That, however, as I reassure myself, is the true attitude to take. It is a far cry to certainty. As Duclaux has aptly put it, the reason why science advances is that it is never sure of anything. Thus I justify my effort of to-day.

Notwithstanding this inadequate treatment of the subject of surface tension in relation to cellular processes, I hope I have made it in some measure clear that the same force which shapes the raindrop or the molten mass of a planet is an all-important factor in the causation of vital phenomena. Some of the latter may not thereby be explained. We do not as yet know all that is concerned in the physical state of solutions. The fact, ascertained by Rona and Michaelis, that certain sugars, which neither lower nor appreciably raise surface tension in their solutions, condense or are absorbed on the surface of a solution system, is an indication that there are at least some problems with a bearing on vital phenomena yet to solve. Nevertheless, what we have gained from our knowledge of the laws of surface tension constitutes a distinct step in advance, and a more extended application of the Gibbs-Thomson principle may throw light on the causation of other vital phenomena. To that end a greatly developed science of microchemistry is necessary. This should supply the stimulus to enthusiasm in the search for reactions that will enable us to locate with great precision in the living cell the constituents, inorganic and organic, which affect its physical state and thereby influence its activity.

A. B. Macallum

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THE EIGHTH INTERNATIONAL ZOOLOGICAL CONGRESS IN GRAZ

In the week before the congress members inspected the biological station in Lunz, with its glass houses and ponds, the lower, middle and upper lakes, the last 1,117 meters high, and were shown the methods of research and some of the results obtained. In Vienna, the great Museum of Natural History, the zoological laboratories of the university and the vivarium were visited. The vivarium, under the direction of Dr. Przibram, is a remarkable institution for work in experimental biology and evolution. There are series of rooms in which the temperature, light and other conditions of existence may be under control, and

aquaria, caves, breeding-pens and laboratories, fully equipped for the investigations in hand.

Graz, the capital of Steiermark, lies on both banks of the Mur, in the region between the eastern Alps and the Pannonian lowlands. In the midst of the quaint old houses rises the Schlossberg, a conical, rocky hill, which, as a natural fortress, has formed a center for the successive struggles of the Celts, Romans, Slavs and Germans. Napoleon had his headquarters here, but in the century of peace since the French occupation Graz has developed into a beautiful and prosperous city of 170,000 inhabitants.

The imposing new buildings of the university, erected in 1890–94, in the newer quarter, have an exceptional setting in a campus of green lawns with shrubs and trees. Flanking the main building are laboratories for physics, natural history, chemistry and medicine, and in the rear, the library. In the various lecture-rooms and laboratories, the headquarters and the sections of the eighth International Zoological Congress were housed during the meeting from August 15 to 20, 1910. Of the 559 members and associates given in the list published August 16, 61 are Americans.

In the mornings the general sessions were held in the fine new Stephanien-saale in the heart of the city. Upon the opening of the congress addresses were made by President v. Graff, Count Stürgkh, minister of culture and education, Count Attems, governor of Steiermark, Dr. Franz Graff, mayor of Graz, Professor Dr. Kratter, rector of the University of Graz, and Professor E. Perrier, president of the permanent committee of the International Zoological Congress. At all times the indefatigable president, v. Graf, won every heart by his genial manner. He referred feelingly to his predecessor, the lamented Agassiz, to whom, in so large measure, was due the wonderful success of the Boston meeting. The address in memory of Anton Dohrn, by Professor Boveri, was characterized by deep sentiment and eloquence, and the proposal to erect in honor of Dohrn a monument near the Zoological Station in Naples met with a hearty approval.

The committee on nomenclature held many meetings and accomplished much work of general importance. For the presentation of papers there were eleven sections: I., Cytology and Protozoology; II., Anatomy and Physiology of the Invertebrata; III., Anatomy and Physiology of the Vertebrata; IV., Embryology; V., Experimental Zoology; VI., Zoogeography and Paleontology; VII., Faunistics and Œcology; VIII., Symbiosis, Parasitism and Parasites; IX., General Taxonomy and Nomenclature; X., General Physiology and Histology; XI., Animal Psychology.

In the program 139 papers and demonstrations are listed and of these the following 33 are from American zoologists:

R. B. Bean, "The Ear as a Morphologic Factor in Racial Anatomy."

S. S. Berry and E. L. Mark, "Luminous Organs in a Cephalopod."

R. S. Breed, "Cellular Elements in Cows' Milk."

E. C. Case, "Recent Discoveries of Permian Reptiles in Texas."

A. H. Clark, (1) "The Recent Crinoids of the Coasts of Africa," (2) "Strict Priority in Zoological Nomenclature; an Appeal to the Workers."

E. G. Conklin, "The Effects of Centrifugal Force on the Polarity and Symmetry of the Egg."

H. E. Crampton, "The Principles of Geographical Distribution as Demonstrated by Snails of the Genus *Partula* Inhabiting Southeastern Polynesia."

C. L. Edwards, "The Idiochromosomes in *Ascaris*."

C. E. Eigenmann, "The Fishes of the High Plateau of British Guiana."

H. H. Field (exhibit), "Die Bibliographien des Concilium Bibliographicum."

A. I. Goldfarb, (1) "Studies on the Influence of Lecithin on Growth," (2) "Studies in Non-regenerating Animals—Study First, The Adult Frog."

E. R. Gregory, "Observations on the Water-vascular System of *Echinarachnius parma* (Sand-dollar)."

G. S. Huntington, "Das lymphatische System der Säuger vom Standpunkt der Phylogenese."

D. S. Jordan, "The Natural History of the Fur Seal of Bering Sea."

W. S. Kellicott, "A Contribution to the Theory of Growth."

T. G. Lee, (1) "Demonstration of Microscopic

Slides showing Implantation of Certain North American Rodents," (2) "Early Stages in the Development of Certain North American Rodents."

G. Lefevre, "Reproduction and Parasitism in the Unionidæ" (joint authorship with W. C. Curtis).

J. A. Long and E. L. Mark, "Maturation of the Egg of the Mouse."

R. S. Lull, "The Armored Dinosaur, *Stegosaurus ungulatus*, Recently Restored at Yale University."

A. G. Mayer, "The Relation between Ciliary and Neuromuscular Movements of Animals."

C. S. Minot, "Comparison of the Early Stages of Vertebrates."

C. F. W. McClure, "Demonstration of a Series of Models, Based on Reconstructions, Illustrating the Development of the Jugular Lymph Sacs in the Domestic Cat (*Felis domestica*)" (presented by G. S. Huntington and C. F. W. McClure).

J. P. Munson, "Organization and Polarity of Protoplasm, Centrosome, Aster and Sphere in Ovarian Eggs, Yolk-nucleus and Vitelline Body."

C. E. Porter, (1) "Les trachées de l'Acanthinoder a cummingi Hope," (2) "Sur quelques Crustacés du Chili."

H. S. Pratt, "Trematodes of the Gulf of Mexico."

O. Riddle, "Experiments on Melanin Color Formation; a Refutation of the Current Mendelian Hypotheses of Color Development."

R. A. Spaeth and E. L. Mark (demonstration), "Chromosomes in Certain Copepods."

C. R. Stockard, "The Experimental Production of Various Eye Abnormalities; and an Analysis of the Development of the Parts of the Eye."

R. T. Young, "Cytology of Cestoda."

Excursions to the museums and other points of interest were conducted by young ladies of Graz. In the unique Landes-Zeughaus of 1642 are 30,000 pieces, including weapons and armor, still seen in the rough racks as originally placed for the use of the soldiers.

On one evening an outing was taken in the Hilmwald, where beside the lake, overhung with hundreds of Chinese lanterns, the Abendessen was partaken of. The beauty of the many-colored lights reflected from the surface of the water, and the quaint folk-music of a band of peasant minstrels added much to the truly Austrian sociability of the evening.

On another occasion the members of the congress lunched together under the trees of a restaurant garden upon the Schlossberg and then enjoyed the views of the distant Alps, beyond the plain of Graz, through which winds the Mur, and nearer, the richly colored roofs intersected by narrow streets and the great city park, with its splendid trees.

Fitting telegrams and addresses were made in celebration of the eightieth birthday of His Majesty the Emperor of Austria and the King of Hungary. Among the excellent responses to the toasts at the final banquet those of President Jordan and Professor Blanchard may be characterized as especially felicitous. After the adjournment of the congress 120 members took part in the excursion to Triest and thence by special steamer along the mountainous coast of Dalmatia.

C. L. Edwards

SCIENTIFIC NOTES AND NEWS

Professor William M. Davis, of Harvard University, has been elected a corresponding member of the Berlin Academy of Sciences.

The Thomas Young lecture before the Optical Society was delivered in the lecture hall of the Chemical Society, London, on September 29 by Professor R. W. Wood, of the Johns Hopkins University. The subjects were "The Echelette Grating" and "The Mercury Telescope."

The Advisory Public Health Board of the Public Health and Marine-Hospital Service was called to meet in Washington, October 10, in view of the cholera in Europe. This board is composed of Drs. Simon Flexner, New York City; Dr. William T. Sedgwick, Boston; Dr. Victor C. Vaughan, Ann Arbor; Dr. Frank F. Wesbrook, Minneapolis, and Dr. William H. Welch, Baltimore.

The Department of State has selected the following delegates to the International Conference on Tuberculosis, to be held in Brussels: Dr. Reid Hunt, of the U. S. Public Health and Marine-Hospital Service; Dr. Mazyck P. Ravenel, Madison, Wis.; Dr. Arnold C. Klebs, Chicago, and C. H. Baldwin, Washington, D. C.

President Henry Fairfield Osborn, of the American Museum of Natural History, has

been appointed honorary curator of the department of vertebrate paleontology and Dr. W. D. Matthew has been promoted to the position of acting curator.

IN continuation of the program outlined some time ago, the Wistar Institute of Anatomy and Biology has taken up the Chemical Study of the Nervous System. The work, in cooperation with Professor H. H. Donaldson, has been put in charge of Dr. Waldemar Koch, of the University of Chicago. Dr. Koch retains his connection with the university but will spend part of his time in Philadelphia and the results will be published jointly from the department of neurology of the Wistar Institute and the laboratory of pharmacology of the University of Chicago.

DR. C. F. CLARK, assistant agronomist in the New York State College of Agriculture at Cornell University, has accepted a position in the Bureau of Plant Industry in connection with the sugar beet investigations.

ARTHUR H. ESTABROOK, Ph.D. (Hopkins), will spend the winter in research work at Cold Spring Harbor, Long Island.

DRS. GODDARD and Spinden, of the department of anthropology of the American Museum of Natural History, attended the Congress of Americanists in Mexico City after which Dr. Spinden again took up his work among the Rio Grande Pueblo of New Mexico.

MISS ALICE C. FLETCHER, Thaw fellow in the Peabody Museum of Harvard University, presented a paper on "The Archeological Activities in the United States," before the Section of Anthropology at the Sheffield meeting of the British Association. Miss Fletcher was elected a vice-president of the section.

PROFESSOR G. H. PARKER, of Harvard University, delivered a lecture on "Taste and Smell," at a meeting of the American Academy of Dental Science, held in Boston, on October 5.

AT the stated meeting of the American Philosophical Society on October 7, Dr. John Chalmers Da Costa read a paper on "Suicide."

AMONG the courses of public extension lectures offered by Columbia University is one on "The Science of Zoology, Fundamentals of Biology and Principles of Evolution" by Professor Henry E. Crampton.

AMONG the public introductory lectures to be given at University College (University of London) during October, *Nature* quotes the following: October 3, "Niton: one of the Argon Series of Gases," Sir W. Ramsay; October 4, "The Origin of Scenery," Professor E. J. Garwood; October 6, "The Life and Times of Sennacherib," Dr. T. G. Pinches; "Recent Investigations into the Mental Growth of Children," Dr. C. Spearman; October 10, "Climatic Control," Professor L. W. Lyde; "Instinct," Professor Carveth Read; October 13, "Experimental Phonetics," Mr. D. Jones.

THE College of the City of New York has acquired, as already announced, the complete private library of the late Professor Simon Newcomb, consisting of about 4,000 volumes and 7,000 pamphlets dealing with astronomy, mathematics and physics. Both pamphlets and books are being catalogued and are now accessible to research students, in accordance with the expressed desire of the professor and Mrs. Newcomb.

A STATUE in memory of Dr. Victor Cornil, formerly professor of pathological anatomy at Paris, has been dedicated at Cusset, his native city.

MME. PASTEUR, widow of Louis Pasteur, whom she assisted in his researches, has died at the age of eighty-four years.

DR. JOHN E. MATZKE, head professor of Romanic languages in Stanford University, died very suddenly at Mexico City on September 18. He had gone to that city as the representative of Stanford University on the occasion of the opening of the Mexican National University.

DR. OTTO LÜDECKE, associate professor of mineralogy at Halle, has died at the age of sixty years.

Mr. Hormuzd Rassam, known for his Assyrian explorations, died on September 16, at the age of eighty-four years.

The deaths are also announced of Dr. Zdenko Ritter von Skraup, professor of chemistry at Vienna; of M. Maurice Lévy, professor of mechanics in the Collège de France and inspector general under the government of roads and bridges, and of Dr. Fulgence Raymond, Charcot's successor in the chair of nervous diseases at the Saltpêtrière and eminent for his contributions to pathological anatomy and psychology.

Members of the American Association for the Advancement of Science who contemplate contributing to the program of Section D are requested to send early notice of their intentions and if possible the titles of their papers to the secretary of the section, G. W. Bissell, East Lansing, Mich. The vice-presidential address by Dean J. F. Hayford will discuss "The Relation of Isostasy to Geodesy, Geology and Geophysics." It is proposed to devote at least one session of the meeting to aeronautics and related subjects and papers along this line are especially desired.

The fourth International Congress for the Care of the Insane will be held at Berlin from the third to the seventh of October.

The fifth International Dairy Congress, which will be held in Stockholm in 1911, offers a prize of £20 for the best essay on the nutritive value of raw milk as compared with that of pasteurized, sterilized or evaporated milk, determined, at least in part, by experiments made upon infants.

The results of a series of tests on the strength of pure iron alloyed with nickel and copper made during the last five years in the applied electrochemistry laboratory of the University of Wisconsin are presented in a new bulletin in the engineering series by Professor Charles F. Burgess and James Aston. Professor Burgess discovered a simple method for producing chemically pure iron electrolytically, and received a grant of several thousand dollars from the Carnegie Institution at Washington with which to carry on the investigations. The value of alloys of nickel with iron, copper with iron, and of nickel and copper with iron is considered in detail in a series of tables, and the methods used in making and testing these combinations are fully discussed in the bulletin.

Dr. E. C. Pickering, director of the Harvard College Observatory, announces that a new star, whose approximate position is R. A. $17^h\ 52^m\ 15^s$, Dec. $-27°\ 32'.3$ (1875), was discovered by Mrs. Fleming in the Constellation Sagittarius, on October 1, 1910. It appears on 16 photographs taken at Arequipa with the eight-inch Bache and one-inch Cooke telescopes, between March 21, 1910, and June 10, 1910. The magnitude has been estimated as varying from 7.8 to 8.6, between these dates. The spectrum is quite faint but shows the bright hydrogen lines $H\beta$, $H\gamma$, $H\delta$, $H\epsilon$, $H\zeta$ and $H\eta$, with a trace of $H\gamma$ as dark on the edge of greater wave-length of the bright line $H\gamma$. The star does not appear on seventeen photographs, taken between July 23, 1889, and October 7, 1909, although most of them show stars fainter than the twelfth magnitude and one plate shows stars of the fifteenth magnitude, or fainter. An observation by Leon Campbell on October 3, 1910, with the 24-inch reflector of this observatory confirms the presence of this object and gives its magnitude as obout 10.5. Of the fifteen new stars known to have appeared during the last twenty-five years, eleven have been found at this observatory, nine by Mrs. Fleming from the photographs of the Henry Draper memorial.

The ordinary meetings of the Royal Geographical Society for the session 1910–11 begin, as we learn from the London *Times*, on November 7, when Major Molesworth Sykes will give an account of his further journeys in Persia. Major Sykes will deal, among other subject, with a tour in ancient Parthia. At the second meeting of the society, on November 21, Dr. H. A. Lorentz will give an account of his recent explorations in Dutch New Guinea. The subject deals to a large extent with a region in which an English ex-

pedition is at present at work, under the leadership of Mr. Goodfellow. There has been considerable activity in the exploration of Dutch New Guinea recently by the Dutch themselves, both from the south and from the north. On November 28 Dr. Filippo de Filippi will lecture on some of the more important results of the Duke of the Abruzzi's last expedition to the Karakoram. No doubt one of the most interesting papers of the session will be that on December 19, by Dr. J. B. Charcot, on the results of his recent Antarctic expedition. Another paper of special interest will be that of January 16, 1911, when Sir John Murray and Dr. Hjort will give a detailed account of the "Michael Sars" North Atlantic deep-sea expedition.

THE Department of Agriculture has issued a set of fifteen charts on the composition of food materials; these charts are printed from photo-lithographs in six colors, and show in the case of each material the protein, fat, carbohydrate, ash and water contents and the fuel value expressed in calories. The percentage composition and fuel value are given in figures and the relative proportion of each constituent is represented graphically. For example, in the case of whole milk a glass of milk is shown; 87 per cent. of the figure is colored green to represent the water content, 3.3 per cent. red to represent the protein, 4 per cent. yellow to represent the fat, 5 per cent. blue to represent the carbohydrates and 0.7 per cent. drab to represent the ash content. The fuel value of 310 calories per pound is represented by printing in solid black nearly one third of a square one inch on each edge, since one square inch represents 1,000 calories. The figures given for the percentage composition of the various materials are average figures based upon as many analyses as are available in each case. The food materials shown in these charts are as follows: 1, whole milk, skim milk, buttermilk and cream; 2, whole egg, egg (white and yolk), cream cheese and cottage cheese; 3, lamb chop, pork chop, smoked ham, beefsteak and dried beef; 4, cod (lean fish), salt cod, oyster, smoked herring and mackerel (fat fish); 5, olive oil, bacon, beef suet, butter and lard; 6, corn, wheat, buckwheat, oat, rye and rice; 7, white bread, whole wheat bread, oat breakfast food (cooked), toasted bread, corn, bread and macaroni; 8, sugar, molasses, stick candy, maple sugar and honey; 9, parsnip, onion, potato and celery; 10, shelled bean (fresh), navy bean (dry), string bean (green) and corn (green); 11, apple, dried fig, strawberry and banana; 12, grapes (edible portion), raisins (edible portion), grape juice (unfermented), canned fruit and fruit jelly; 13, walnut, chestnut, peanut, peanut butter and cocoanut. Chart 14 gives the functions and uses of food under the headings, "Constituents of Food" and "Uses of Food in the Body." Chart 15 shows the dietary standard for a man in full vigor at moderate muscular work and the estimated amount of mineral matter required per man per day. These charts are printed on sheets 21 by 27 inches of a good quality of paper, and are for sale by the Superintendent of Documents, Government Printing Office, Washington, D. C. The charts will be found especially useful to instructors and students in classes in physiology, domestic science and other branches in which the food and nutrition of man is studied, either in schools or colleges or in clubs or similar organizations.

THE annual report of the registrar-general for Ireland, issued as a Blue-book and summarized in the London *Times*, shows that the excess of births over deaths in 1909 was 27,786, and that the loss by emigration amounted to 28,676, which was greater by 5,381 than in 1908, but less than the average number—37,141—for the ten years 1899–1908. There would, according to these figures, appear to have been a decrease of 890 persons in the year 1909. With regard to immigration there is no official record, nor does it enter into the estimate of the population to the middle of the year, which was 4,371,570—an increase of 115 on the estimate for the previous year. According to the last quarterly returns of the registrar-general, the population of Ireland in the middle of this year was 4,371,133. The population has therefore been practically stationary for three years. The marriages regis-

tered in Ireland during 1909 numbered 22,650, the births 102,759, and the deaths 74,973. The marriage rate was 5.18 per 1,000 of the estimated population (a decrease of 0.02 as compared with that for 1908, but an increase of 0.06 against the average rate for the ten years); the birth-rate was 23.5 per 1,000 (0.2 above the preceding year and 0.3 above the average); and the death-rate 17.2 per 1,000 (0.4 below the previous year and 0.6 below the average). An estimate of the progress of elementary education was formed from the signatures made by the contracting parties in the marriage registers or certificates. In 1909 93.5 per cent. of the husbands and 95.0 per cent. of the wives wrote their names, the remainder signing by marks, as against 86.8 and 88.6 per cent. in 1899, 78.8 and 78.0 per cent. in 1889, and 72.0 and 67.1 per cent. in 1879.

UNIVERSITY AND EDUCATIONAL NEWS

The Sproul Observatory, of Swarthmore College, is nearing completion. The telescope, which will have a twenty-four-inch aperture, is being constructed at Allegheny, and will probably be installed this coming year. In the same building will be installed a new refracting telescope, the gift of Mr. Stephen Loines, of New York.

The Tuskegee Institute will receive about $400,000 from the estate of Mrs. Dotger, and the Hampden Institute will receive about $250,000 from the estate of Miss Alice Byington.

By the death of Mrs. Mary Hunt Loomis, the estate of the late Colonel John Mason Loomis, amounting to more than $1,000,000, will, it is said, go to the establishment of a technical school at Windsor, Conn.

The Supreme Court has granted an injunction to the stepchildren of the late George Crocker, restraining the executors from selling the property which was bequeathed to Columbia University for a cancer research fund.

Bryn Mawr College will celebrate the twenty-fifth anniversary of its opening on October 21 and 22. Among the speakers will be President Remsen, of the Johns Hopkins University, and President Lowell, of Harvard University.

Dr. Charles C. Harrison, provost of the University of Pennsylvania, has tendered his resignation to the board of trustees, to take effect at the end of the present academic year.

The following promotions and appointments have been made in the chemical department of the University of Illinois: Edward Bartow, professor of analytical chemistry; C. W. Balke, assistant professor of inorganic chemistry; E. W. Washburn, assistant professor of physical chemistry; instructors, Ellen S. McCarthy (Ph.D., Cornell), C. G. Derick (Ph.D., Illinois), Paul E. Howe (Ph.D., Illinois); research assistants, Josef Hecht (Ph.D., Vienna); assistants, H. P. Corson (N. H.), J. H. Mitchell (Ala. Poly. Inst.), C. J. Baker (Univ. of Denver); graduate assistants, H. B. Gordon (Miami), H. H. Radcliffe (Ind. Univ.), G. E. Ostrom (Augustana), N. R. Blaterwick (Grinnell), D. W. Wilson (Grinnell), C. P. Sherwin (Ind. Univ.), E. L. Ross (Iowa State Agr. College), J. H. Bornmann (Illinois); fellows, S. J. Bates (McMaster Univ.), J. W. Read (Missouri), A. A. Schlichte (Michigan), L. R. Littleton (Tulane); graduate scholars, P. S. Burgess (R. I. State College), G. W. Sears (Drury College).

Dr. C. C. Grove has been appointed assistant professor of mathematics at Columbia University.

Mr. H. Bateman, fellow of Trinity College, Cambridge, and lecturer in mathematics in the University of Manchester, has accepted an appointment in the department of mathematics of Bryn Mawr College.

J. F. Daniel, Ph.D. (Hopkins), has been appointed instructor of comparative anatomy at the University of California.

Edith M. Twiss, A.B. (Ohio State University, 1895), Ph.D. (Chicago, 1909), has been appointed assistant professor of botany with charge of plant physiology and bacteriology at Washburn College, Topeka, Kansas. For some years Miss Twiss has taught in the Cleveland High Schools.

THOMAS M. HILLS, Ph.B. (Wooster), and a recent graduate student in the University of Chicago, has been appointed assistant professor of geology in the Ohio State University.

CHARLES B. WILSON, Ph.D. (Hopkins), has been appointed professor of biology at the State Normal School, Westfield, Mass.

DISCUSSION AND CORRESPONDENCE

AMŒBA MELEAGRIDIS

TO THE EDITOR OF SCIENCE: Nearly two years ago there appeared in this journal a communication by Drs. L. J. Cole and P. B. Hadley,[1] concerning the etiology of a protozoan disease of turkeys which demands some notice on my part.

The disease in question was investigated by me in 1894 and described in detail in a bulletin of the Bureau of Animal Industry, U. S. Department of Agriculture which was published in 1895. The disease is confined to the two cæca and the liver. Minute round bodies not more than 8–12μ in diameter appear in enormous numbers in the submucous and intramuscular tissue of the walls of the cæca and may extend even beyond these to the mesenteries. In the liver there are circular spots, representing partial necrosis of the liver tissue and in these spots the same organisms are also present in great numbers. This parasite I assumed to be an amœba and called it *A. meleagridis*. The analogy between it and human amœbiasis was very close.

In the communication of Drs. Cole and Hadley, my interpretation of the parasite is promptly disposed of and the latter stated to be a stage in the life history of the common coccidium of fowls and other domesticated and wild birds. This coccidium has been known since Rivolta first described it in 1878. Though I felt grave misgivings concerning the position taken by these writers, I nevertheless refrained from expressing my views until a full report should have appeared. In the meantime my patience has been tried by repeated iterations of the statements in various journals, scientific and practical, without any offer of proof that their position had any basis in fact. At last two and a half years after their preliminary statement a bulletin[2] appears.

As an illustration of the way "facts" will grow when unchallenged I select the following statements from preliminary papers:

> Since the investigations of Theobald Smith published in 1895 it has been commonly believed that the disease [blackhead] is due to an amœba, *A. meleagridis* Smith. The present writers *believe*[3] they have *demonstrated*, however, that the disease is caused by a coccidium which according to the nomenclature adopted may be a variety of *C. cuniculi* and that *A. meleagridis* is probably the schizont stage in the development of the coccidium.[4]

> The *discovery* that the so-called blackhead of turkeys so common in this country is a form of coccidiosis (SCIENCE, 1908, N. S., XXVII., p. 994) and that the causative organism *C. cuniculi* is one of the most important factors in the causation of the so-called white diarrhœa of chicks and of some cases of roup in fowls, has called the attention of the student of protozoology in this country to the presence of a protozoan parasite whose ravages are annually costing the country hundreds of thousands of dollars.[5]

These excerpts speak for themselves. A "belief" becomes a "discovery" a year later, although no published data accompany the belief or precede the discovery. The discovery consists in fitting together two parasites both regarded as distinct for many years. Furthermore, the avian coccidium is identified with the rabbit coccidium without proof. It is made the "most important factor" of a diarrhœal disease of chicks and of roup in fowls, also without proof. Roup has defied many investigators and is due probably to an invisible virus.

The full report now before us confirms my suspicions that the demonstration and discovery represented merely an inference or hypothesis. Yet upon this the report is built as if it were an assured fact. Nothing whatever

[1] 1908, N. S., Vol. XXVII., p. 994.

[2] No. 141, Rhode Island Agric. Exp. Station.

[3] Italics mine.

[4] Cole and Hadley, SCIENCE, 1908, N. S., XXVII., p. 994.

[5] Hadley in *Centralbl. f. Bakt.*, Erste Abth. Orig., 1909, 52, p. 147.

has been added to existing knowledge, and the expensive work done in the form of experiments is worthless to future investigators, because the authors have failed to keep apart ordinary coccidiosis and the parasite producing the specific cæcal and liver lesions. Even though subjectively convinced of the truth of their hypothesis, they should have objectively recorded the lesions and kinds of parasites found in the subjects of their experiments, so that others, who refuse to accept their hypothesis, might still have utilized the results. We have now a report which is neither one thing nor the other; it is neither on coccidiosis nor on entero-hepatitis.

When I first heard of entero-hepatitis as a "coccidiosis," I went over all the material from cases of the disease then in the laboratory to endeavor to read if possible this new hypothesis into the facts, although I had already stated in my early report (1895) that "it is very improbable that these bodies (coccidia) stand in any genetic relation to the true micro-parasite of the disease." This recent enquiry, however, carried me still farther away from this new hypothesis.

The weakness of the position taken by Cole and Hadley can be easily grasped by readers who are not protozoologists and pathologists when put in possession of a few fundamental facts. It has been the experience of microbiologists for the past thirty years that when a disease which is apparently due to a certain causative organism shows now one type of lesion, now another, now the presence of the suspected organism, now its absence, two infectious agents are involved which may work together or separately.

Whenever microorganisms can not be studied in pure culture artificially the infection with the products of disease may lead to double or even triple infections, because two or even three parasites may be in the infecting material. The same may occur spontaneously in any restricted territory where several diseases have coexisted for years. Most animals living in such locality may become the victims of several diseases. The only way out of the difficulty is to study the disease as it occurs in widely separated localities. If it can be shown that outbreaks of entero-hepatitis may occur without coccidia and that outbreaks of coccidiosis may occur without liver disease and the presence of *A. meleagridis*, we have cleared away most of the difficulties surrounding the interpretation of a dual infection. Let us see what facts we can bring together bearing on this phase of the subject.

In 1894 I examined animals from nineteen farms, but only on two was coccidiosis present. This spring I examined a small flock of young turkeys kindly incubated and reared for me by Dr. Austin Peters. Though six out of nine of this flock died of "blackhead," without being exposed to any disease so far as we can discover, *not a single coccidium* was found either in the diseased or in the healthy animals. By a stretch of imagination it might be claimed that coccidia had not time to mature in these animals, which either died or were killed in from four to ten weeks after hatching. But as I have seen mature coccidia cysts in turkeys four weeks old this argument can not be used.

Although avian coccidiosis has been known since 1878, it is strange that close observers like Rivolta and many subsequent writers fail to report lesions of the liver which are so characteristic of the entero-hepatitis of turkeys. Surely this striking lesion would not have escaped even the most cursory and superficial examination. The authors in their recent report fail to distinguish between coccidiosis of the liver in which the epithelium of the bile ducts is the seat of the invasion, and the embolic, blood infection of the turkey's liver in which the parenchyma alone is affected. I do not recall any description of either type of liver disease in the coccidiosis in birds, although there is no reason why liver coccidiosis might not be found in birds as in rabbits. Leaving, however, aside this important distinction, let us see what the authors say of "coccidiosis" in other birds (on page 180 of their recent report). In four guinea-fowls, coccidia were present in either intestines or cæca, *but there were no liver lesions.*[6] In two out of five ducks, coccidia were present in the cæca *but not in the liver.*

[6] Italics mine.

Three pheasants were infected with coccidia but the livers are not mentioned. In two quail the typical lesions of blackhead were present in intestines and liver, the organism being found both in the *tissues* (?) and the intestinal contents. In one grouse coccidia were found. The liver is not mentioned.

Of seventeen pigeons all of which died, some with symptoms of coccidiosis, the organisms were found in nine and were usually accompanied by such lesions of either intestines or liver that a diagnosis of coccidiosis was justifiable. In several of the other eight pigeons, lesions which resembled those of *blackhead* were found both in intestines and liver, but apparently *not accompanied by coccidia.*

Sjöbring,[7] who studied coccidiosis among birds in Sweden, describes forms belonging to two genera of coccidia. The one, evidently the predominating if not the only one observed by Cole and Hadley and by me, was found by Sjöbring in pheasants. The other, characterized by the presence of two instead of four spores, was encountered in many different species of birds. The author states distinctly that he found neither kind in the liver.

Since the writer's work in 1894 the entero-hepatitis of turkeys has been encountered in the common fowl. It seems as if this parasite of turkeys had adapted itself to fowls and to other species of birds. In the above quotations from Cole and Hadley's work we see enough uncertainty to make us believe that the authors saw now one disease, now the other, now both together in different birds without distinguishing between them.

There is thus ample evidence to show that entero-hepatitis may run its course in a flock without the presence of a single coccidium cyst to suggest coccidiosis. On the other hand, it is evident that coccidiosis among birds has been frequently seen during the past thirty years, but without involvement of the liver. Finally a double infection seems to have been the rule at the Rhode Island Experiment Station, where the work of Cole and Hadley was done and where the animals used in the experiments were reared.

[7] *Centralbl. f. Bakt.*, Erste Abth., 1897, 22, p. 675.

This simple fundamental statement must suffice for the present. Aside from this there are many reasons why *A. meleagridis* and *C. tenellum* should not be regarded as identical. The former organism has no morphological characters which even remotely suggest a coccidium, and its situation and mode of attack upon the tissues are likewise wholly different from those which accompany coccidiosis. To state more than this would require a minute analysis of many pages of text in which the writers have laboriously endeavored to explain why true coccidia are met in some cases and not in others. If we should try to describe kangaroos and zebras intermingling in an enclosure, now in terms of one, now in terms of the other by assuming a genetic relationship between them, we would be in the same predicament in which the authors find themselves. To attempt to correct matters would be impossible.

It is obvious that in pathological work it is important to distinguish between lesions of different character, for they are of great service in the study of causation. In biological research it is far more important to keep morphological entities apart than to throw them together, unless very good reasons appear for identifying them. It is always possible for our successors to put them together, whereas a separation is impossible when a single term such as "blackhead" or coccidiosis is used to cover all. Rivolta had the same problem before him when first describing avian coccidiosis.[8]

In 1873 he noticed in the intestinal wall of fowls, dead of disease or killed, white points, the size of a poppy seed, found in the submucous connective tissue. These were small cysts full of "navicellæ" (merozoites?). In 1878 he saw in young chickens a disease, characterized by emaciation, diarrhœa, pallid flesh, etc., and by the presence of large numbers of minute white points in the duodenum. They appeared to be in the submucosa. In the intestinal contents many oval psorosperms (coccidia cysts?) were found. Rivolta

[8] "Della gregarinosi dei polli, etc.," *Giorn. di anat. fisiol. et patol. degli animali*, Pisa, 1878, X., p. 220.

rejects the identity of submucous cysts and psorosperms for the following reasons:

1. The psorosperms always inhabited the epithelial cells, the gregarines the submucous connective tissue.

2. There were fowls which contain thousands of psorosperms but no gregarines.

3. There were found young chickens, blackbirds and crows with gregarinosis without showing any psorosperms.

Rivolta's example might well be followed by our younger scientists. It is easier for the time being to make all forms over into a single species but in the end it is likely to lead to nothing. Rivolta, by the way, says nothing of liver lesions.

Another instance of the possible presence of two distinct parasites constituting what has for eighteen years been regarded as one, has recently been discussed by A. Theiler.[9] Theiler thinks that what has hitherto been regarded as a single blood corpuscle parasite in Texas cattle fever represents two. In the first report[10] on this disease both forms were shown to appear in the blood of cattle which had received a single injection of blood from a southern animal. Both live within the red cells, one type appearing first in the course of the disease, then the other. Theiler argues with much force that there are two species involved because in some parts of the world one type alone was reported as present in the blood of diseased animals, in other parts, the other type. In our own country both types occur. Without accepting for the moment Theiler's views, which I have not yet studied in detail, I think they are suggestive and worth careful attention. Fortunately in our report these types have been noted separately in the protocols, so that even after eighteen years the records are available for an analysis of Theiler's position.

Among the other blemishes of a work which otherwise shows much industry and study and a commendable care in editing is the use of the term *Coccidium cuniculi* and the suggestion that there is any direct relation between the coccidium of the rabbit and that of birds. To assume that a species which refuses to invade near mammalian relatives and which seems to cling to the rabbit host throughout the world should have a closer relationship or even be identical with the avian coccidium seems to be attributing to nature a fickleness which students of parasitism know only too well does not exist. So clearly defined and narrow is the range of parasites even in the same host that it is with difficulty that coccidia locate in the epithelium of the large intestine when the epithelium of the upper small intestine has been preempted. The statement should therefore have been based on some actual experiments on birds with *C. cuniculi* of the rabbit.

In order to avoid misunderstanding in making this criticism, I wish to state emphatically that I do not regard my early work as in any sense complete. The questions concerning the amœbic character[11] of the bodies I described, the simple or complex nature of their life cycle, the direct, indirect or intermediate mode of infection do not come into consideration. Whatever position concerning one and all of them I had taken may be disputed as long as the life cycle has not been satisfactorily worked out. The final solution of these questions can be reached only after years of experimental breeding and rearing in carefully guarded territories on which no poultry is kept and from which even game and other wild birds are excluded. My criticism is confined to the confusing of an old well-known with a new and poorly known protozoan parasite and the consequent uselessness of the investigation as a basis for further work. I also wish to protest against the publication of premature, undigested, controversial statements in the form of preliminary notices years before the appearance in print of the actual work on which such statements are presumably based.

Theobald Smith

Harvard Medical School,
September 20, 1910

[9] *Ztschr. f. Infektionskrankheiten d. Haustiere*, etc., 1910, 8, p. 39.

[10] Smith and Kilborne, "Investigations into the Etiology of Texas or Southern Cattle Fever," Washington, 1893.

[11] Amœbic changes in form have been noted recently in liver tissue examined *immediately after chloroforming affected turkeys.*

WINCHELL ON OPHITIC TEXTURE

TO THE EDITOR OF SCIENCE: In the proceedings of the twenty-first annual meeting of the Geological Society of America, Volume 20 of the *Bulletin*, pages 661 to 667, Professor A. N. Winchell has a paper upon the use of ophitic and related terms in petrography. Since I in my report for 1909 shall continue to use the term in a somewhat narrower sense than that advocated by Professor Winchell,[1] a few words of explanation may not be out of place. I shall not plead that publication of the paper was too late to be availed of since Professor Winchell was kind enough to let me read it some time ago. Nor is the argument that one should not change his usage in what may perhaps be the last of my reports of entirely determining weight, though in view of the fact that what I have called ophites Winchell would also call ophitic, the point has a certain weight. The facts regarding the early and later use of the term ophitic are fully given by Winchell in the article referred to, with perhaps one exception. That is, in the article from which Winchell cites the original definition of Michel-Lévy in the *Bulletin of the Geological Society of France*, Volume 6, 1878, page 158, only a few pages later (on page 169) he says, "the most characteristic mineral of the ophites is the diallage in the large areas." It seems to me, therefore, very questionable if one should extend the term so as to apply it as Winchell suggests "to all rocks having plagioclase in lath-shaped crystals of earlier formations." In fact, it seems to me the petrographically and chemically important thing is the fact that the rock has pretty nearly the composition of a bisilicate and that this bisilicate may be considered as the solvent in which the other constituents are dissolved, from the fluid or molten solution of which they crystallize. One finds, for instance, in the quartz diabases, rocks in which the plagioclase is distinctly in lath-shaped crystals of early formation, but in which the matrix is not pyroxene. It seems to me that, as cited by Winchell in the earlier or later definition, a pyroxenic matrix is an essential part of the idea of the ophites.

I am, however, quite willing to give up the idea that the augite must necessarily be altogether in larger grains than the feldspar. In fact, in almost all the so-called ophitic rocks at a proper distance not far from the margin one will find a transition from a glassy intersertal or microlitic texture to the coarse ophitic texture, in which the augite acts as matrix to the feldspar, but is so fine grained that several granules may combine in acting as a matrix for a single feldspar. Now this structure would certainly be covered by the original definition as cited by Winchell, in which the size of the augite is not emphasized. But the fact of a pyroxenic matrix seems to me essential to the idea. The extension to a rock in which the pyroxene is replaced by native iron is perhaps an extension by analogy.

ALFRED C. LANE

[1] The same sense in which it is used by the list of writers cited by him, to which may be added Grout, in SCIENCE for September 2, 1910, p. 313.

THE REFORM OF THE CALENDAR

TO THE EDITOR OF SCIENCE: The suggestions of Professor Reininghaus and Doctor Slocum concerning the reform of our present calendar, which were published in SCIENCE for June 29 and September 2, are very pertinent and interesting. It is certainly time for some international action looking to the reform of our clumsy calendar. In this connection I beg leave to call attention also to a plan for the reform of the calendar presented last year to the first Pan-American Scientific Congress by Sr. Carlos A. Hesse, of Chili. He suggests the division of the year into thirteen months of 28 days each, the new month to follow December and be called Trecember. The extra day (for $13 \times 28 = 364$ only), he proposes to call "Zero Day," and it would not belong to any of the fifty-two weeks, or be called by any week day. The extra day in leap years he proposes to call "Double Zero Day," under like conditions. This project is nearly that suggested in the letters in SCIENCE referred to above, except that Dr. Slocum's plan (which he ascribes to Mr. Moses B. Cotsworth, of York, England) is to place the extra month in the *middle* of the year instead of at

the end, and name it the month of Sol, while the suggestion of Professor Reininghaus is that the extra month be divided into two fortnights, one to follow after June and be called the "summer half-month," and the other to come at the end of the year and be called the "winter half-month." Just why this latter scheme should be, as Professor Reininghaus claims, more "practical" than to keep the extra month intact, is difficult to see.

After studying the various schemes offered, the following plan would seem the most feasible:

1. Adopt the arrangement of 7 days to the week, 4 weeks (28 days) to each month, and 13 months plus 1 extra day (in leap years 2) to the year.

2. Place the extra month in the middle of the year between June and July. It should not be named Sol, because in the southern hemisphere the month would come in the dead of winter, and the name would be a misnomer. No name borrowed from the old French Revolutionary Calendar (*e. g.*, Thermidor) would be applicable either, for the same reason. It might be better to name the new month Rome or Roma, in tribute to the city where both the Julian and the Gregorian Calendars originated, or else give it a name meaning "mid-year." The objection to placing this extra month between December and January is that there would be such a gap between Christmas Day and New Year's Day, and Christmas would be thrown forward entirely out of a winter month.

3. Call the extra day New Year's Day, and do not apply to it the name of any week day. The objections to having Christmas as the extra day are that it does not come as the initial or final day of the year, and many persons, such as orthodox members of the Jewish Church, might reasonably object to such a unique distinction being given to Christmas Day. Non-Christian nations would probably object, too, and as any reform of the calendar should be such as would be internationally acceptable, it would be well to forestall all objections, if possible.

4. Begin every month with Monday. The same monthly calendar would then be repeated over and over throughout the year, and every one would know by memory the days of the week corresponding to the days of the month. Wall calendars would be absolutely unnecessary except in primary schools.

5. Call the second extra day in leap years "Leap Day," and let it follow New Year's Day.

6. It is rather a fortunate coincidence that according to this plan nearly all of our fixed national and state holidays would come on days other than Sunday: February 12, February 22, March 4, April 19, July 4, October 12, Thanksgiving Day, Christmas Day and many others.

Of course some rearrangement would be necessary with some of them. There are really three kinds of holidays or festivals to be looked after: (*a*) movable feasts, such as those of the church; (*b*) fixed dates, such as Christmas, All Saints' day, etc., which shift automatically with any change in the calendar, and (*c*) celebrations of certain *days*, not dates. For example, Washington was born on February 11 (see the entry in his mother's Bible at Mt. Vernon), but as this was the same *day* in the old style calendar which we now call February 22, we celebrate the latter day. Perhaps some of the dates in class (*c*) above mentioned would be shifted for the same reason in a reformed calendar (Washington's birthday itself, for example), but the dates belonging to classes (*a*) and (*b*) would take care of themselves.

7. This proposed calendar would, of course, bring about the occurrence of the vernal equinox several days later than March 21, but it is unlikely that the old controversy over this matter started at the Council of Nicæa and settled in the sixteenth century would again arise.

8. Any possible confusion in changing calendars would be avoided if at the same time the method invented by Scaliger in 1582 for harmonizing all systems of chronology is thoroughly explained to the people in general. According to this system each day has a num-

ber, beginning with an era now nearly seven thousand years ago. For example, January 1, 1911, will be Julian Day 2,419,038. The interval between any two dates, one reckoned by the old calendar, the other by the new, may be easily found when their Julian numbers are known, and these may be found or calculated from almanacs.

9. It is greatly to be hoped that, if a reform is made in the calendar, we shall adopt the plan of naming the hours in the day up to 24, so as to avoid the useless writing of A.M., P.M., M., and the like, after the hour. In Italy, for example, this simple plan is followed with the best results.

ANDREW H. PATTERSON
UNIVERSITY OF NORTH CAROLINA,
CHAPEL HILL

SCIENTIFIC BOOKS

Physical Science in the Time of Nero: Being a translation of the Quæstiones Naturales of Seneca. Pp. liv + 368. London, Macmillan & Co. 1910.

As the work of the most distinguished thinker, and writer of his time, the "Quæstiones Naturales" of Seneca (3–65 A.D.) commands attention; and as a landmark in the progress of human knowledge, it is of permanent interest. In this volume of 368 pages, the Roman philosopher did for his day what Aristotle had done four centuries earlier in his physical and meteorological treatises. Seneca records the observations of previous writers, adds many of his own and discusses all from the lofty plane of the philosopher and moralist.

This was only natural, as there was no school of experimental science in Athens, Alexandria or Rome in the lifetime of Seneca. Indeed, many a century had to pass before the inquirer into the phenomena and laws of nature condescended to measure and weigh, to use his hand as well as his intellect.

The Greek mind had for abstract truth a marked fondness which was unfavorable to such drudgery as manipulation; the Roman, while less subtle and more practical, also showed a decided preference for general observation and philosophical speculation.

Aristotle and his disciple Theophrastus were the authoritative masters of the physical knowledge of Greek and Roman antiquity; and to them Seneca frankly acknowledges his indebtedness. But if from their pages on meteorology, astronomy and physical geography, he borrows the substance of some of his chapters, a perusal of the seven books which compose the "Quæstiones Naturales," will show that he has a clear way of describing the phenomena of nature and an insistent way of presenting his explanations and defending his opinions regarding them.

In the original, the work was divided into eight books which, in course of transcription, was reduced to seven by the union (probably) of Books II. and III.

Book I. treats of the rainbow, halos and mock suns; Book II., of lightning and thunder; Book III., of the forms of water; Book IV., of snow, hail and rain; Book V., of winds and general movements of the atmosphere; Book VI., of earthquakes, and Book VII., of comets.

In discussing the rainbow, Seneca remarks that it may be seen at night as well as during the day, provided the moon is unusually bright, to which he adds that the rainbow colors are the same as those which are seen by holding a glass rod obliquely in the path of the sun's rays. The magnifying power of a spherical water-lens did not escape his observant eye; for he says that "letters, however small and dim, are comparatively large and distinct when seen through a glass globe filled with water."

In treating of earthquakes, he recognizes three kinds of movements, viz., the *quaking* "when the earth is shaken and moves up and down"; the *tilting* "when, like a ship, it leans over to one side," and the *quivering* when "no great damage is usually done." He also adds the just observation that maritime districts are those which are most frequently shaken.

In his book on comets, he affirms that a comet is not "a sudden fire, but one of nature's permanent creations"; and he does not hesitate to berate one Ephorus for saying that a certain great comet which had been "carefully watched by the eyes of the whole world and which drew issues of great moment in its

train" broke up and "was resolved into two parts."

Ephorus may have been right despite the caloric statement of Seneca; for, in our own times, we have witnessed the disruption of Biela's comet and have assurance of the disintegration of scores of others.

One is not surprised to read of nature's abhorrence of a vacuum; but even an ardent admirer of Seneca would hardly expect to find a reference to the doctrine of the conservation of matter (p. 121) or to the effect of forest denudation on the amount of rain-fall and on the character of floods (p. 122).

Though the rotation of the earth upon its axis and its revolution around the sun had been advanced by several Greek astronomers to explain the phenomena of day and night Seneca seems to cling to the old belief of a stationary earth and a revolving starry dome.

The "Quæstiones Naturales" was written in the last year or so of a life that was busy intellectually and troublous politically; for if Nero was a docile student, he showed himself afterward an ungrateful pupil as well as a ruthless tyrant. One may well wonder how Seneca found the time and tranquility needed to add the present scientific treatise to his numerous writings dramatic, philosophical and moralistic.

Throughout these pages, Seneca shows a keen appreciation of the value of observation for the extension of our knowledge of the world around us, and also of the importance of common sense in the interpretation of our observations.

To this translation in fine literary English, Professor Clarke has prefixed a life (54 pages) of the Roman sage, and Sir Archibald Geike, President of the Royal Society, has appended a valuable analysis (23 pages) of each of the seven books. This critical analysis from a master pen gives by itself a good idea of what was known in physical science in the time of the Emperor Nero.

Brother Potamian

Manhattan College

Allen's Commercial Organic Analysis. Vol. II., Fixed Oils, Fats, Waxes, etc. Fourth edition, entirely rewritten. Edited by Henry Leffmann and W. A. Davis. Philadelphia, P. Blakiston's Son and Co. 1910. Pp. x + 520. Price $5.00 Vol. III., Hydrocarbons, Asphalt, Phenols, Aromatic Acids, Modern Explosives. Pp. x + 635. Price $5.00.

As with the first volume, which was reviewed in Science a few months ago, these volumes have been so entirely rewritten as to form practically new books. As with that the different chapters have been written by experts in the different fields. In Volume II. the authors are: Fixed Oils, Fats and Waxes, C. Ainsworth Mitchell; Special Characters and Methods (Olive Oil Group, Beeswax, etc.), Leonard Aschbutt; Butter Fat, Cecil Reeves and E. R. Bolton; Lard, C. Ainsworth Mitchell; Linseed Oil, C. A. Klein; Higher Fatty Acids, W. Robertson; Soap, Henry Leffmann; Glycerol, W. A. Davis; Cholesterols, John Addyman Gardner; Wool Fat, Cloth Oils, Augustus H. Gill. In Volume III., Hydrocarbons, F. C. Garrett; Bitumens, S. S. Sadtler; Naphthalene and its Derivatives, W. A. Davis; Anthracene and its Associates, S. S. Sadtler; Phenols, S. S. Sadtler; Aromatic Acids, Edward Horton; Gallic Acid and its Derivatives, W. P. Dreaper; Phthalic Acid and the Phthaleins, W. A. Davis; Modern Explosives, A. Marshall; Table of Comparison for Centigrade and Fahrenheit Degrees.

The methods of analysis for complex mixtures of organic compounds are almost unlimited in their variety and make use of all kinds of physical and chemical properties. A book which brings together the best of these methods and which is filled with copious references to the literature of the subjects considered is indispensable in every laboratory where such products are examined. This revision of Allen's well-known book under the editorship of Leffmann and Davis and with the collaboration of well-selected experts meets this need excellently.

W. A. Noyes

SCIENTIFIC JOURNALS AND ARTICLES

Terrestrial Magnetism and Atmospheric Electricity for September contains the follow-

ing articles: "Farewell Visit Aboard the *Carnegie,* at Greenport, Long Island, June 27, 1910," frontispiece; "The Circumnavigation Cruise of the *Carnegie* for 1910–13 and the Perfection of Her Magnetic Work as Shown by Recent Tests," by L. A. Bauer; "Magnetic Chart Corrections Found on First Cruise of the *Carnegie,*" by L. A. Bauer and W. J. Peters; "Glossary of Atmospheric Electricity Terms," by W. W. Strong; "Observations of Earth-currents in Stockholm on May 19, 1910, during Passage of Halley's Comet," by D. Stenqvist and E. Petri; "Magnetische Beobachtungen in Seddin Während des Kometendurchangs, 19. Mai, 1910," by A. Nippoldt; "Magnetic Observations at Cheltenham, Maryland, May 15–20, 1910," by R. L. Faris; "The Magnetic Character of the Year 1909," by G. van Dyk; "Cooperation in British Antarctic Expedition, 1910," by J. Larmor; "Principal Magnetic Storms Recorded at the Cheltenham Magnetic Observatory, April–June, 1910," by O. H. Tittmann; "Die Werte der Erdmagnetischen Elemente in Apia, 1905–08," by F. Linke und G. Angenheister.

NOTES ON ENTOMOLOGY

RECENT parts of the "Genera Insectorum" include a continuation of W. Horn's Cicindelidæ, fascicle 82 b, pp. 105–208, plates 6 to 15, mostly colored; a most excellent review of the tiger beetles. Fascicle 100 is on the Pterophoridæ, or plume-moths, by E. Meyrick, 22 pp., 1 plate, colored, and is also a useful review. Fascicle 101 is on the large exotic cockroaches of the subfamily Epilamprinæ, by R. Shelford, 21 pp., 2 colored plates. Fascicle 102 on the ants of subfamily Dorylinæ, by C. Emery, 34 pp., 1 plate. Fascicles 103 and 104 are by L. B. Prout on the geometrid moths of the subfamilies Brephinæ, 16 pp., 1 plate, and Œnochrominæ, 120 pp., 2 plates. The latter group is almost wholly from the old world. Fascicle 105 is on the wasps of family Thynnidæ, by R. E. Turner, 62 pp., 4 plates (2 colored). He makes many new genera, mostly from Australia or South America. Fascicle 106 is on the ortalid flies of the group Ulidini, by F. Hendel, 76 pp., 4 colored plates of these beautiful insects. He describes two new species from the United States, *Euxesta tenuissima* (p. 28) from Georgia, and *Acrosticta rufiventris* (p. 52) from Texas. Fascicle 107 is on the minute hymenopterous parasites of the family Belytidæ, by J. J. Kieffer, 47 pp., 3 plates.

MAJOR THOS. L. CASEY has issued No. 1 of a "Memoirs on the Coleoptera," 205 pp., 1910. This number contains two articles, New Species of the Staphylinid Tribe Myrmedoniini and Synonymic and Descriptive Notes on the Pæderini and Pinophilini. He has described 365 new species, only a very few being identified with known forms. Most of the species are in the genera *Atheta* (which he divides into many subgenera)—*Sableta, Datomicra, Colpodota* and *Strigota.* Many of the species are from the eastern states.

MR. H. B. STOUGH is the author of a detailed study of the external morphology of one of the jumping plant lice.[1] Besides the structure of the body he takes up the wing-venation and color-pattern. He finds that the media and cubitus in nymphal wings are distinctly separate. From the structure of mouth and genitalia he concludes that the Psyllidæ are more closely related to the Aleurodidæ than to any other family of insects.

DR. G. ALESSANDRINI has made some experiments with the larvæ of *Piophila casei,* known as the cheese-skipper.[2] These larvæ can pass through the digestive tract of man or dog without greatly delaying development. In a dog the larvæ produced lesions of the intestine which facilitated the entrance of pathogenic germs. The larvæ can resist the action of many chemical agents, but the ultra-violet rays retard development. The life-cycle occupies about fifty days.

E. WASMANN continues his observations on

[1] "The hackberry Psylla, *Pachypsylla celtidismammæ* Riley, A Study in Comparative Insect Morphology," *Kans. Univ. Sci. Bull.,* V., No. 9, pp. 121–165, 10 pls., 1910.

[2] "Studi ed Esperienze sulle larve della *Piophila casei,*" *Arch. Parasitol.,* XIII., pp. 337–382, 33 figs.

the ants of Luxemburg.[3] This part contains the species of *Camponotus, Formica* and *Polyergus.* There is a large amount of biological matter about each species, but the plates, which are photographs, do not well illustrate the structure of the species.

The fifth volume of Theobald's monograph of the Culicidæ or mosquitoes of the world has been issued by the British Museum. It contains 646 pp., 6 pls. and 261 text figures. There are 392 species recorded since volume four was issued, not many of them new; most of the additions are from Africa or Australia, none from the United States.

W. Wesché has made a new subfamily of crane-flies, the Ceratocheilinæ.[4] It is based on two new genera of small flies from Africa. The proboscis is very long and thin, with short palpi inserted near its apex; the antennæ are short, and the second joint subglobular. The wings are similar to *Ptychoptera;* the claws are simple. One genus, *Ceratocheilus,* bears peculiar bifid hairs on the legs.

About 1892 to 1894 Dr. H. V. Nassonov published, in the Russian language, several large papers on the curious insects known as Strepsiptera. A translation has now been printed in German.[5] In an appendix is a review of the literature on the group since Nassonov's papers.

Part 7 of Kertesz's "Catalogus Dipterorum," 470 pp., includes the Syrphidæ, Dorylaidæ, Phoridæ and Clythridæ. *Clythia* replaces *Platypeza* and *Dorylas* replaces *Pipunculus.* Other 1,800 names of Meigen are used as follows: *Cinxia* for *Sericomyia, Toxomerus* for *Mesograpta, Zelima* for *Xylota, Penthesilea* for *Criorrhina, Lampetia* for *Merodon* and *Tubifera* for *Helophilus.*

[3] "Verzeichniss der Ameisen von Luxemburg mit biologischen Notizen," *Arch. trimes,* 1909, Vol. IV., fasc. 3 and 4, 103 pp., 5 plates.

[4] *Journ. Linn. Soc. London, Zool.,* XXX., pp. 355–368.

[5] "Untersuchungen zur Naturgeschichte der Strepsiptera," by A. v. Sipiagin, with notes by K. Hofender, *Berichte Naturwiss.-med. Vereins, Innsbruck,* XXXIII., pp. 206, 6 pls., 1910.

Dr. F. Ris has published a continuation of the Libellulinæ of the de Selys collection.[6] This part contains the genera *Libellula* and *Perithemis* and allied forms. *Libellula* is used in a broad sense, including *Plathemis* and *Ladona.* The forms of *Perithemis domitia* are considered as species, our common one thus becoming *P. tenera* Say.

Mr. F. Neeracher has made many interesting studies on the insects of the Rhine River that form an instructive paper.[7] He has found 13 species of Perlidæ, 19 of Ephemeridæ and 31 Trichoptera. He gives descriptions of the species, and of the larvæ of many of them. There are notes on male dimorphism, length of adult life, duration of generations, comparative abundance, and the date of first appearance for three consecutive years. He finds that the species with long life as adults appear in the spring, while those of a very short life appear in great numbers, and in mid-summer.

Volume V. of "Fauna Arctica," Jena, 1910, contains two entomological articles: one by J. C. H. de Meijere is on "Die Dipteren der arktischen Inseln," pp. 15–72. The Nemocera, Anthomyidæ and Scatomyzidæ are numerous, but other flies are scarce, and but two species of mosquitoes. The other article is by B. Poppius, "Die Coleopteren des artischen Gebietes," pp. 289–447. He considers the tree-limit as the southern boundary of the arctic fauna. The Carabidæ and Staphylinidæ are particularly well represented. There is a chapter on the geographic distribution of arctic beetles.

A new entomological journal is the *Bulletin of Entomological Research,* apparently a quarterly and devoted to the economic entomology of tropical Africa. It is edited by a committee of English entomologists and pathologists, Mr. Guy A. Marshall being secretary and editor. Parts 1 and 2 of volume I. have been

[6] "Collections Zoologiques du Baron Edm. de Selys Longchamps," Fasc. XI., pp. 245–384, 1 plate, 80 text figures, 1910.

[7] "Die Insektenfauna des Rheins und seine zuflüsse bei Basel," *Rev. Suisse Zool.,* XVIII., pp. 497–589, 1910.

issued, 160 pp. Most of the articles treat of insects injurious to man or animals.

SOME years ago Dr. O. M. Reuter published a system of classification of the hemipterous family Capsidæ. Now he has issued a new arrangement.[3] He has modified his previous classification in various details and made nine subfamilies. He gives a list of the genera, placing most of them in the proper subfamily. The article also includes a review of the classifications of the Heteroptera, and a new one, in which he arranges the 40 families in 12 superfamilies. There are tables to these families and to the groups of the Capsidæ. One of the new features is the elevation of *Piesma* to family rank.

NATHAN BANKS

SPECIAL ARTICLES

THE SELECTIVE ELIMINATION OF ORGANS

ONE of the monuments erected to Charles Darwin on the hundredth anniversary of his birth might have been a bibliographic index to the literature of organic evolution. But it is very much easier to pen a series of addresses on Darwin's method, Darwin's real opinion, Darwin's influence, than it is to compile a comprehensive bibliography and analyze it with the thoroughness and detail and wisdom necessary to make it a really useful aid to the investigator; it would have taken a very plucky librarian (with wealthy friends and a genius for interesting them in his undertakings) to carry it through.

As his card manuscript for the subject index approached completion he would have found that several drawers in his cabinet were required for the cards bearing the caption *natural selection*. These cards would have been a key to everything that can be said in a theoretical way about natural selection. The student who would take these cards and attempt conscientiously to cover the field would be ready, after a year's floundering about in the morass of rhetoric, to believe that all the arguments—for and against—have been presented in all their possible permutations.

That no solid foundation for a scientific superstructure is to be found in this polemic quagmire has often been recognized; at present natural selection is out of fashion among biologists. Other problems are in the searchlight.

It is quite natural that a theory which has been so much talked about but as little investigated should cease to be attractive at a time when concrete experimental proof is so much in demand. But can not such proof be adduced for natural selection? Is it not possible that the biologist of to-day with the powerful tools of statistical analysis at his service may be able to demonstrate the existence of natural selection, just as by the use of these tools he has been able to measure the strength of heredity?

Fortunately a beginning has already been made, for if the index were brought well up to date probably over a dozen of the cards in the drawers devoted to natural selection would bear titles of papers embodying the results of serious attempts to measure the intensity of the selective death rate in some organism.

In the selection theory of evolution—the pure Darwinian theory as popularly conceived—there are three factors which must be not only existent, but coexistent, if there is to be any shift in the characteristics of succeeding generations of any organism. These factors are variation, inheritance and selective elimination. If any one of these be absent or its force counterbalanced by some other factor, Darwinian evolution in that species can not be taking place at the moment in question.

Now a great mistake of most of the men who have written on organic evolution has been that they have tried to solve the whole problem. Lacking data (or having only a modicum of data), they have invoked assumptions and logic, and, having proved their assumptions by their logic, have proceeded to generalizations. In dealing with a problem

[3] "Neue Beiträge zur Phylogenie und Systematik der Miriden," *Acta Soc. Sci. Fenn.*, XXXVII., No. 3, 1910, pp. 171.

of so great complexity we should keep constantly in mind the fact that it is idle to attempt to untangle the whole snarl at once. Rather we should try to study each factor intensively, isolating it when possible from others and measuring its force. If in doing this we find that variations do occur and in abundance, we have demonstrated an important physiological fact, but a fact without significance in Darwinian evolution unless the variations be both heritable and some of them of such superior utility that they have an advantage over others in the struggle for existence. Again the demonstration of the inheritance of any character does not yield conclusive evidence on Darwinian evolution unless accompanied by a proof of its selective value. Finally it is quite conceivable that a stringent selection should recur every generation without effecting any change in a species—such being necessary to maintain the type in its present condition or without significance because acting upon characters not inherited.

In the face of these difficulties only one course is open to the naturalist: to spend much time in the potting shed and the breeding pen, to be strenuous in the use of the eye piece micrometer, the calipers, the color scale, the statistical tables and the calculating machine; to believe that ten times the conventional number of observations are desirable; to repeat his experiments and to make new series of measurements; and to believe that a few gourds full of statistical constants with tabulated data from which they may be verified are more to be desired than an artesian well of personal opinion based on non-quantitative observations.

The results outlined in this essay are drawn from recent contributions to these gourds full of quantitative data. The tabulated observations and the detailed analysis from which others may verify these statements if they choose are to appear in a forthcoming number of *Biometrika*.

By a selective elimination one understands that the members of a population do not die at random, but that some individuals are, because of innate physical, physiological or psychical peculiarities, much more likely to die[1] than others.

Theories may be spun concerning the relationships of any character whatever to natural selection, but for purposes of scientific investigation one must limit his attention to those which are directly or indirectly measurable. Illustrations of the directly measurable characters are to be found in Bumpus's sparrows, Weldon's crabs, Crampton's moths and Weldon's and di Cesnola's snails.

The characteristics of an individual are the sum of the characteristics of several organs: probably it is the fitness of these organs which largely determines whether the individual shall be able to survive and leave the average number of offspring or more. But suppose that each individual produces a great number of organs, only a small fraction of which become matured and functional. Might it not be possible to determine from some measurable character of such organs whether failure to develop to maturity is due to any characteristic of the organ—in short, whether there is a selective elimination of organs? Ideal material for such investigation is found in some of the flowering plants. A large number of ovaries are formed, of which only a small per cent. develop to maturity. There is a large elimination: to determine whether this is a selective elimination, whether those which survive to maturity differ in any measure from those which die and fall from the plant, is our problem.

The American bladder nut, *Staphylea trifolia,* has a fruit with three cells in each of which from four to a dozen ovules are formed. The number of the ovules can be counted in the ovary of the opened flower and in the matured fruit. Only a small proportion of the ovaries formed reach maturity and by com-

[1] In sexual selection the elimination would occur as a failure to mate. In reproductive selection it would occur as a relatively lower capacity for producing offspring. Here only elimination involving death—in our present material the death of an organ—is taken into account.

paring samples of those which fall from the tree with those which ripen, one can judge whether elimination has been in any degree dependent upon the number or arrangement of ovules in the locules. Naturally conclusions to be valid must be based upon a very large series of countings, and to be quite sure that the differences are not obscured by heterogeneity of material, the ovaries of each individual should be treated separately. In the spring and summer of 1908, about 7,000 ovaries (involving the opening and counting of 21,000 locules) were taken from twenty-eight individuals in the North American tract of the Missouri Botanical Garden. These were in three series; a sample of flowers which fell from the tree when it was shaken gently and which had therefore ceased to develop and were ready to fall from the tree, a sample of those which remained, and, finally, a collection of the matured fruits later in the summer.

The second collection represents probably most nearly the condition in the original population of pods; it contains some which would have developed to maturity and some which would have fallen from the tree later. The most critical comparison for the detection of selective elimination is that of the eliminated with the matured ovaries. This is the comparison which will be chiefly employed in these pages.[2]

The conviction that there must be a selective elimination of ovaries came to me through an extensive biometric study of fertility in various kinds of fruits. The immediate suggestion for the detailed investigation begun in 1908 was furnished by a small series of developing ovaries of *Staphylea* collected for quite a different purpose in the spring of 1906. These fell into three length groups, 5–10, 11–15 and 16–20 mm. If selective elimination really occurs one would expect the third series, which has most nearly reached maturity, to differ sensibly from the second and especially from the first. The results from this series are in general agreement with those for the 1908 collections, although the method in which they were made prevents their being strictly comparable. The difference in method emphasizes the soundness of the conclusions drawn.

Changes in Mean due to Selective Elimination.—In any investigation of natural selection the first step is to ascertain whether a difference in the size of organs or in the number of parts can be demonstrated between those individuals which are eliminated and those which survive. Concretely, for our present problem, are ovaries with many or ovaries with few ovules best fitted to become functional?

Diagram 1 makes very clear the differences in the average number of ovules per locule for the 1908 series. The arrows show that in 27 out of the 28 individuals the result of the elimination has been to raise the average number of ovules per locule by the elimination of those with lower numbers. The amount of difference in the mean of eliminated and matured ovaries is shown by the length of the shaft for the individual shrubs and by the two transverse lines for the combined collections. The broken line shows the mean for all the eliminated, and the solid line the mean for all the matured, ovaries. The difference between the two is pronounced. Arithmetically it is

Average for eliminated ovaries	$= 7.2355 \pm .0092$
Average for matured ovaries	$= 7.7474 \pm .0080$
Difference	$= .5119 \pm .0121$

Absolutely the difference is only half an ovule, but the number of observations on which this average is calculated is so large that the probable error of the difference is small and its trustworthiness very great. Relatively the difference represents an increase of no less than seven per cent. in the number of ovules per fruit.

Looking at the diagram again, we note that individuals differ widely among themselves in the lengths of the arrow shafts—the amount of the difference between the eliminated and

[2] All comparisons are worked out in the original memoir, which must be consulted for details.

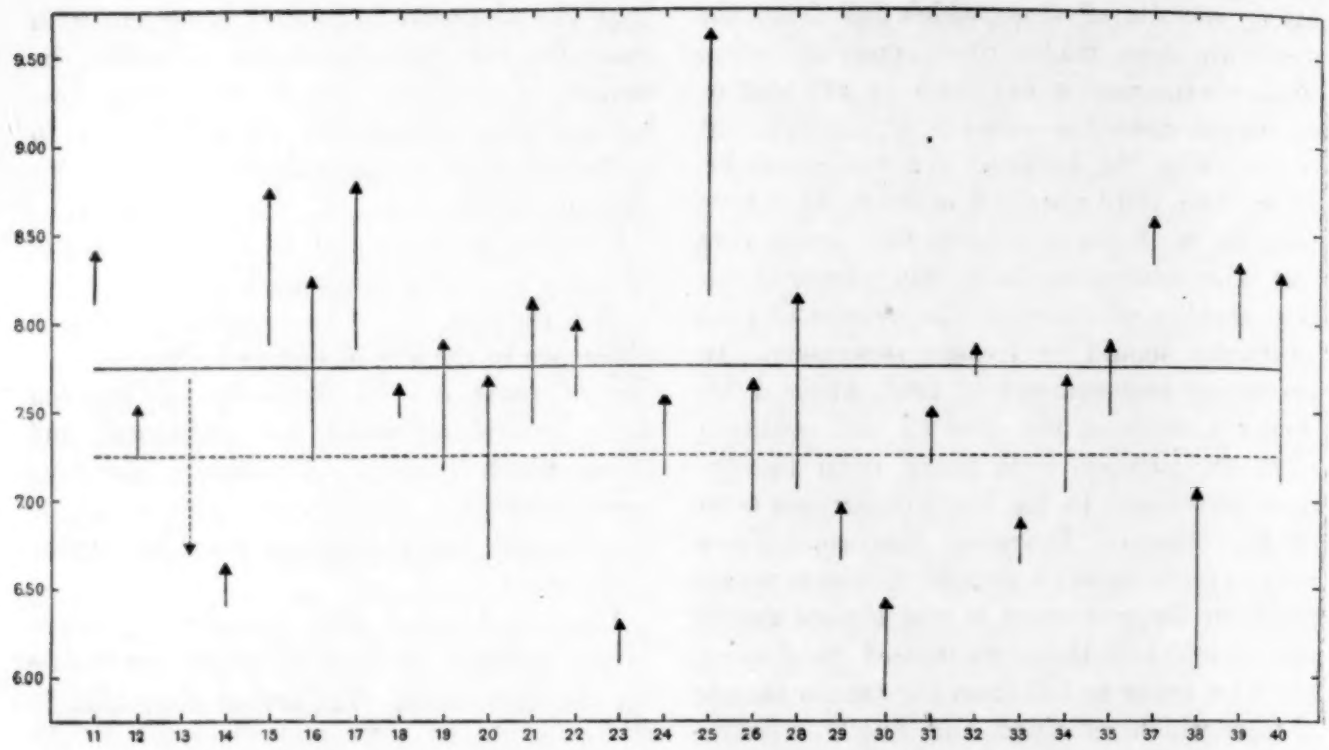

DIAGRAM 1. Showing change in mean number of ovules due to selective elimination for each of twenty-eight individuals.

the matured ovaries. There are probably two reasons for this. The first is that there is a real difference between individuals in this respect. The second is that a part of the variation is due to the probable errors of the constants.

Both constants, based as they are upon random samples—and in the case of the individuals, not very large samples—are subject to the limitations of a probable error of random sampling.[3]

It is interesting to inquire how many of the differences for individual trees may be regarded as trustworthy. Following the conventional standard, we find one significantly negative difference, two which are positive and probably significant, and twenty-five which are positive and unquestionably significant.

From the 1906 series in which the ovaries were taken in different stages of development, we find confirmatory evidence. We have for average ovules per locule:

Series A = 6–10 mm. ovaries, Av.	= 7.232 ± .029
Series C = 16–20 mm. ovaries, Av.	= 7.821 ± .016
Difference	= .589 ± .033

Here the difference is nearly eighteen times its probable error, and represents an increase in mean number of locules of over eight per cent. of the mean for the youngest fruits.

From the foregoing facts only one conclusion can be drawn. In *Staphylea* the failure of ovaries to develop to maturity is not random but selective. Ovaries with lower number of ovules have smaller chances of becoming fruits than those with higher numbers. The intensity of selection is such that there is a difference of about seven per cent. in the mean number of eliminated and matured ovaries.

Changes in Variability due to Selective Elimination.—The comparisons for variabil-

[3] This is the plus or minus quantity attached to the means given above. Conventionally a difference between constants is considered trustworthy if it is two and a half times its probable error. The difference of the two general-series means given above is over forty times its probable error.

ity in the number of ovules per locule and per fruit are somewhat more complicated than those for mean number, so the reader need not be burdened with details.

On the whole it seems that the variability of the matured fruits is less than that of the original series of ovaries before elimination has taken place. In the 1906 series, where we are working with ovaries in different stages of development, there seems to be a steady decrease in the variability as we pass from the youngest to the oldest.

In the 1908 collections the eliminated organs also seem to be less variable than the original series. Probably this means that those which develop to maturity came largely from the upper end of the original range of variation, while those which fail came chiefly from the lower end. Obviously the variability of a part of a population selected towards a particular mean or type can not equal that of the whole population.

Changes in Mean Radial Asymmetry due to Selective Elimination.—In a fruit of *Staphylea* the numbers of ovules may be the same in all three cells or differ from locule to locule. Opening the compartments quite at random—there being no external characteristic to indicate any difference in them—one may find such numbers of ovules as

11—11—11
10—11—10
8—10— 9
9— 9—11
9— 7—10

and so on.

Now we may consider a fruit in which the ovules are distributed equally among the three locules as radially symmetrical with respect to number of ovules per locule; such are fruits of the type 8–8–8, 9–9–9, 11–11–11. Ovaries with one locule differing from the others by a single ovule, *e. g.*, 9–8–9, are somewhat radially asymmetrical, while those with all three locules with different numbers of ovules, for instance 9–8–7, are more so.

As a measure of this radial asymmetry we may take the mean square deviation of the number of ovules per locule from the mean number in the whole fruit. For a fruit of the type

$$\begin{matrix}(a) & (b) & (c)\\ 7 & - & 8 & - & 6\end{matrix}$$

the mean number per locule is 7 and we have:

$$(A-a)^2=0$$
$$(A-b)^2=1 \qquad \text{Coefficient of asymmetry}$$
$$(A-c)^2=1 \qquad \sqrt{2/3}=.8165$$

For an ovary of the formula 7–8–7, $A=7.333$, $(A-a)=+.3333$, $(A-b)=-.6666$, $(A-c)=+.3333$, and the coefficient of asymmetry is

$$\sqrt{\frac{.3333^2+.6666^2+.3333^2}{3}}=.4714.$$

The asymmetries of the fruits studied in 1908 ranged from .0000 to 2.1602. To determine whether there is a selective elimination depending upon the radial asymmetry of the fruit as just defined, we obtain the coefficient for each individual ovary and compare the means of those in the eliminated series with those which develop to maturity. Diagram 2 constructed in the same manner as that for the means shows the result for the individual trees. The arrows show that in seven cases the mean asymmetry is greater after elimination has taken place, while in twenty-one cases it is less. The two transverse lines show that for the grand totals there is a very decided reduction in asymmetry as we pass from the eliminated to the matured ovaries. Statistically the differences are:

Mean asymmetry of eliminated ovaries
$=.4515 \pm .0051$
Mean asymmetry of matured ovaries
$=.3724 \pm .0045$
Reduction in asymmetry by selective elimination
$=\overline{.0791} \pm .0068$

Absolutely the difference is not large, but relatively it appears that there has been a reduction of $(.0791 \times .100)/.4515=17.5$ per cent. The difference is more than ten times its probable error and highly reliable.

For the developing ovaries taken in 1906

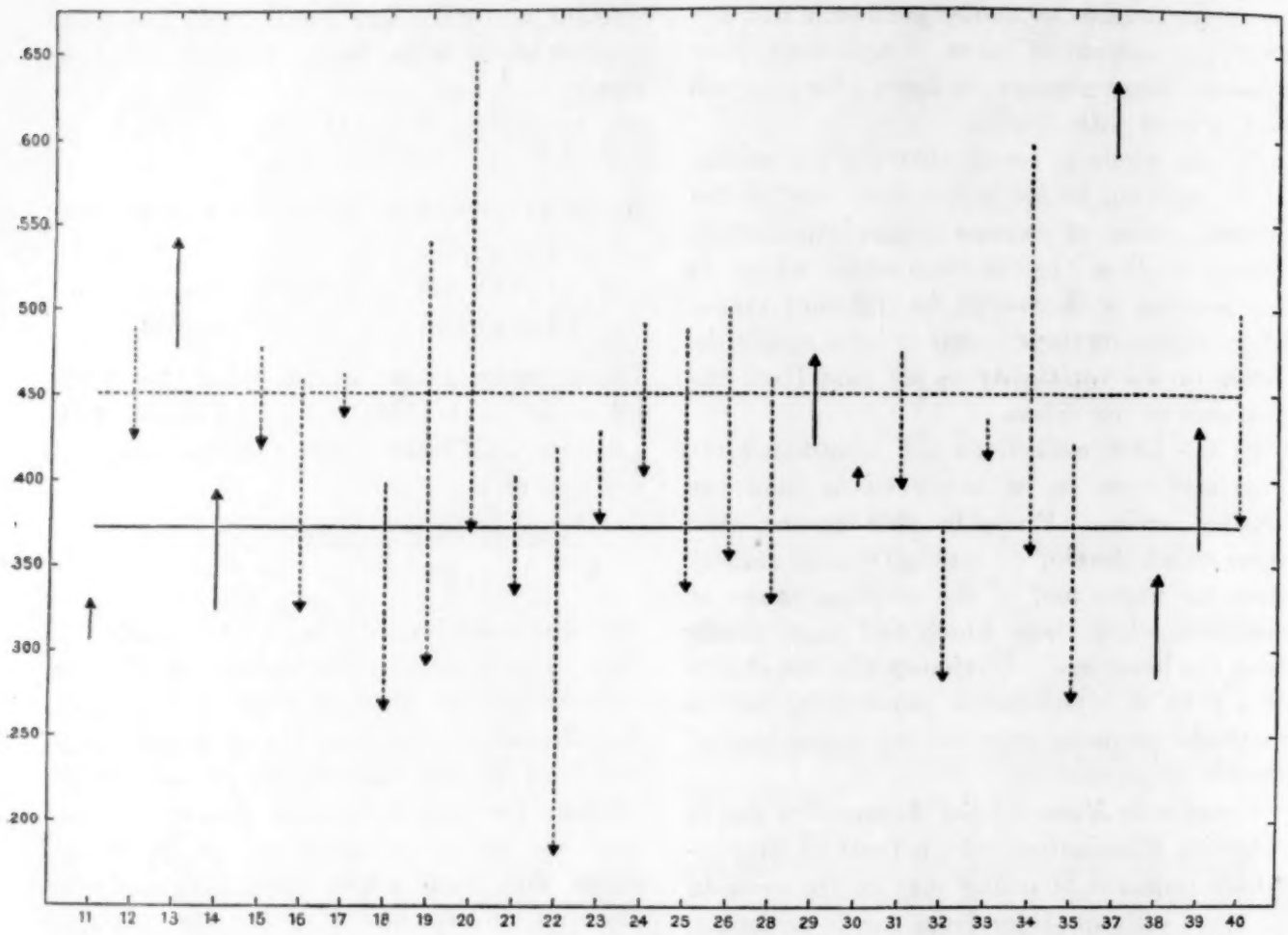

Diagram 2. Showing change in radial asymmetry due to selective elimination for each of twenty-eight individuals.

the differences in asymmetry between largest and smallest are:

Average asymmetry of 6–10 mm. ovaries
$= .382 \pm .016$

Average asymmetry of 16–20 mm. ovaries
$= .302 \pm .013$

Reduction in asymmetry by selective elimination
$= .080 \pm .020$

This result, a decrease of 20.9 per cent. in asymmetry, agrees with the preceding, but owing to the smallness of the series the probable errors are relatively large.

The differences in the eliminated and matured ovaries appear in the frequencies of the individual asymmetry classes. Grouping together the relatively few asymmetries of 1.2472 and over, reducing the frequencies of both eliminated and matured ovaries to a percentage basis, we obtain diagrams 3 and 4. Here the areas with horizontal shading represent the frequency of eliminated ovaries or of the youngest ovaries, while the vertical shading shows the frequency of matured fruits or of the most mature fruits for each of the five asymmetry classes.

The conclusions to be drawn are as obvious as in the case of the mean number of ovules. The failure of pods to complete their development is not a matter of chance, but there is a selective elimination in which the proportion of radially asymmetrical fruits is very greatly reduced. The ovaries which survive to maturity are much more symmetrical than those which are unable to complete their development.

Changes in Locular Composition due to Selective Elimination.—The number of ovules in *Staphylea* varies from about four to about thirteen. Locules with numbers such

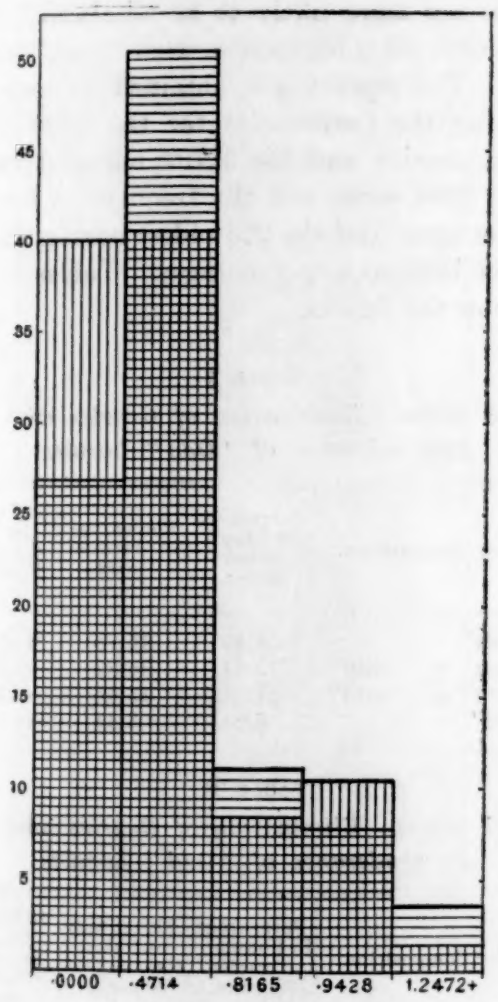

DIAGRAM 3.

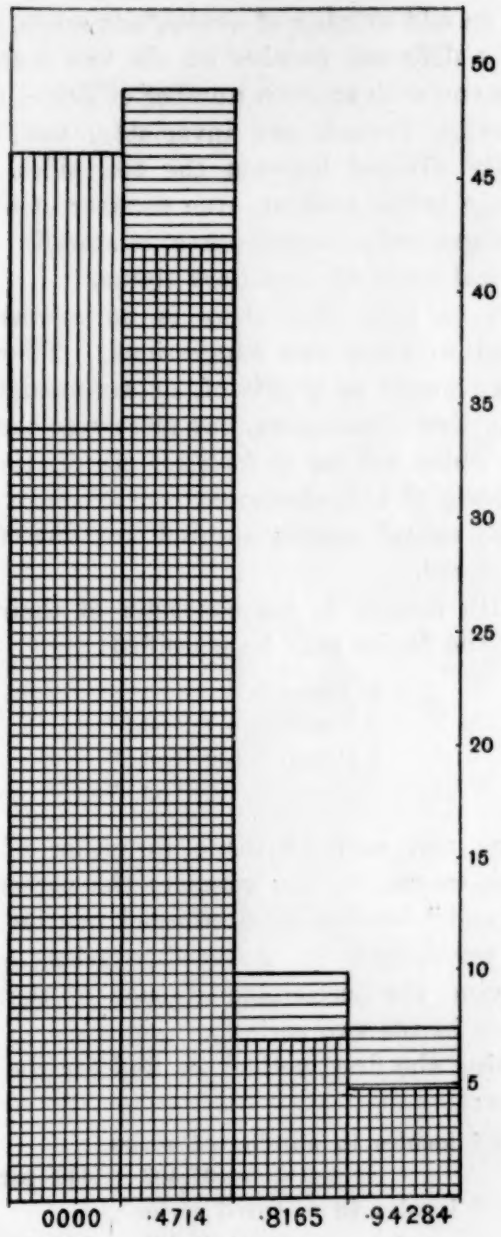

DIAGRAM 4.

as 4, 6 and 8 are conveniently designated as "even" while those with the numbers 5, 7, 9 and so on are tersely described as "odd."

The suggestion that these two types of locules may differ in their capacity of developing to maturity is not the product of the mathematician's fancy, but the working hypothesis sequent upon a large mass of biological observations. The biological reasons for determining whether there is a selective elimination of fruits with a preponderance of "odd" locules are two.

First: I knew from large and numerous series of statistical data that there are in some species of plants differences in the relative numbers of "odd" and "even" locules in the matured fruits.

Second: The ovules in the flowering plants are generally borne on the two margins of a carpellary plate. The ovary is formed by the fusing of the two margins of the embryonic plates, either with each other or with the margin of the adjoining plate. So far as I am aware the development of the ovary of *Staphylea* has not been investigated, but judging by analogy with other forms, it would seem that each locule represents a single carpel with two ovule-bearing margins fused together.

If this be true it is clear that a carpel

with an odd number of ovules,[4] say seven, must have a different number on the two margins, while one with an even number of ovules quite generally, though not invariably, has them equally divided between the two sides. In short, a locule with an even number of ovules more generally represents a bilaterally symmetrical plate of carpellary tissue.

Let us hope that there is no mistake in regard to these two suggestions. They are not advanced as proofs of, or arguments for, a selective elimination, but merely as the reasons which led me to look for elimination on the basis of this character.

The actual results secured are now to be considered.

With respect to the character of their locules, the fruits may be classified:

3 "even"
2 "even" + 1 "odd"
1 "even" + 2 "odd"
3 "odd"

One test may be made in either of two ways: we may either compare the percentage of "odd" locules in eliminated ovaries with the percentage in matured ovaries, or by studying the percentage of each of the four classes in the two series.

Using the first method we find for the 1908 series:

"Odd" locules in eliminated series
$= 37.407 \pm .412$ per cent.
"Odd" locules in matured series
$= 25.185 \pm .325$ per cent.
Decrease in "odd" locules by selective elimination
$= 12.222 \pm .524$ per cent.

And for the 1906 collections:

"Odd" locules in youngest series
$= 33.91 \pm 1.40$ per cent.
"Odd" locules in oldest series
$= 27.23 \pm 1.05$ per cent.
Decrease in "odd" locules by selective elimination
$= 6.68 \pm 1.75$ per cent.

There can be no reasonable doubt that these differences are significant, and that there is some biological reason why locules with "odd" numbers of ovules are less capable of completing their development than those with "even" numbers.

Naturally one locule can not fail to develop without two others falling at the same time. The final test of our theory is to determine whether ovaries with one to three "odd" locules are more likely to be eliminated than those with all three locules with "even" numbers. The results are obtained at once by reducing the frequencies for the 2,095 eliminated ovaries and the 2,707 matured fruits of the 1908 series and the frequencies for the 174 youngest and the 273 oldest ovaries in the 1906 collections to percentages. Tables I. and II. show the figures.

TABLE I

1908 Series. Elimination of Ovaries with a Preponderance of "Odd" Locules

Locular Composition.	Percentage in Eliminated Series.	Percentage in Matured Series.	Difference Due to Selective Elimination.
3 "even"	28.40	46.78	+18.38
2 "even" + 1 "odd"	37.61	34.32	— 3.29
1 "even" + 2 "odd"	27.35	15.46	—11.89
3 "odd"	6.64	3.44	— 3.20

TABLE II

1906 Series. Elimination of Ovaries with a Preponderance of "Odd" Locules

Locular Composition.	Percentage in Youngest Ovaries (6–10 mm.).	Percentage in Oldest Ovaries (16–20 mm.).	Difference Due to Selective Elimination.
3 "even"	33.33	45.78	+12.45
2 "even" + 1 "odd"	39.08	31.14	— 7.94
1 "even" + 2 "odd"	20.12	18.68	— 1.44
3 "odd"	7.47	4.40	— 3.07

Two things are very prominent in these tables. The first is the fact that in all four series the formulæ with a preponderance of "even" are greatly in excess of those with a preponderance of "odd" locules. For 1908 these are 66 per cent. of the eliminated and 81 per cent. of the matured ovaries. In 1906 they are 72 per cent. of the youngest and 87 per cent. of the most mature ovaries.

No less marked is the fact that all classes of ovaries except those with only "even" locules have become relatively less frequent

[4] I see no reason to doubt it, but of course a working out of the problem on *Staphylea* itself by some embryologist is highly desirable.

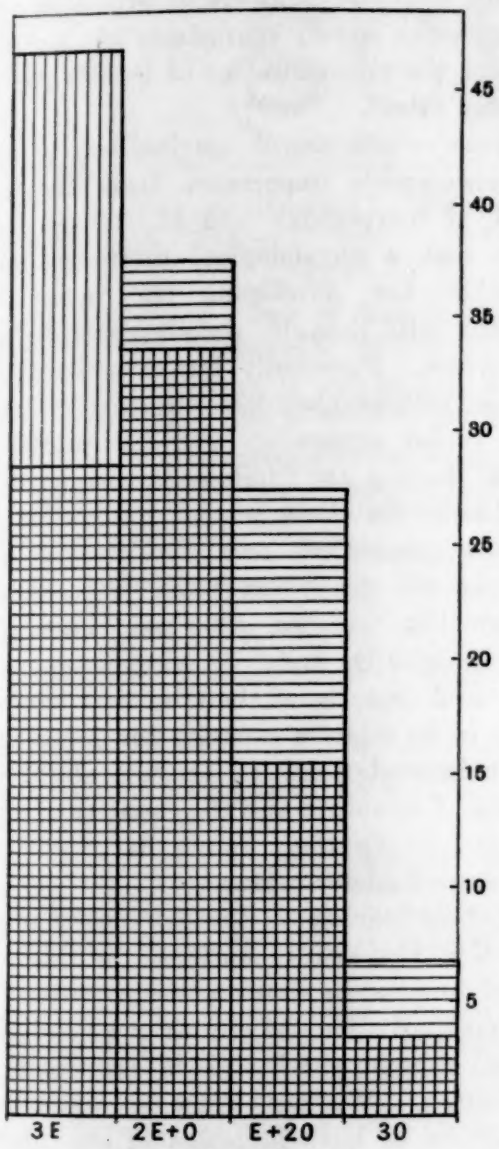

DIAGRAM 5.

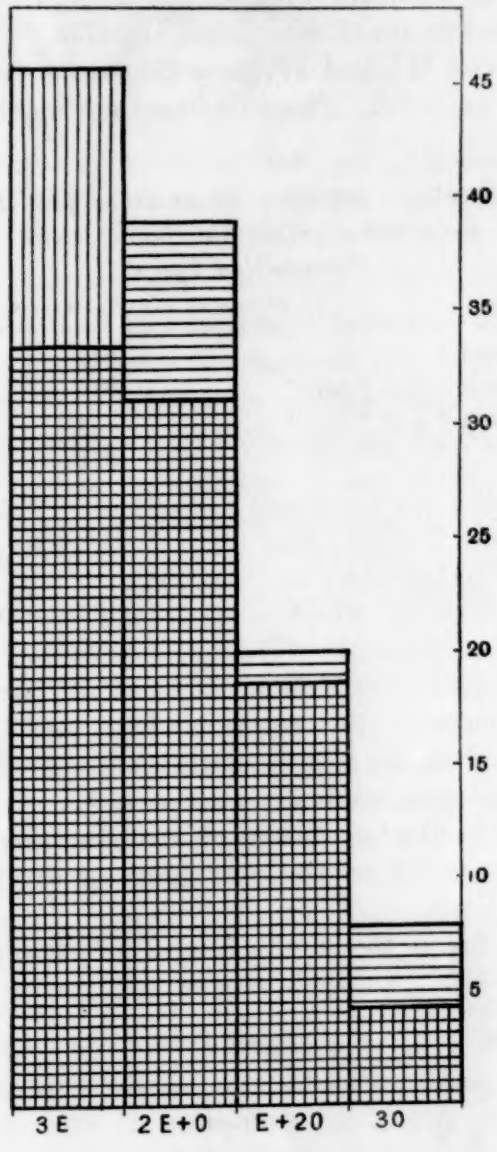

DIAGRAM 6.

by the elimination. Diagrams 5 for 1908 and 6 for 1906 constructed on the same plan as diagrams 3 and 4 make this perfectly clear.

The two characters, radial asymmetry and locular composition, are not independent. The more asymmetrical fruits are, in the long run, somewhat more likely to have more "odd" locules than the average. The intensity of the relationship is shown by a correlation of about .300. In consequence of this condition it is not possible to say without further analysis whether both characters are of selective value, or whether the elimination of ovaries with one of the characters—either the presence of "odd" locules or radial asymmetry—is due merely to the fact that they also possess, in some degree, the other.

If we divide our material up into groups according to both asymmetry and locular composition, and then determine whether within these groups there is a change in the mean value of the other character as we pass from the eliminated ovaries to those which have developed to maturity, we shall, I believe, be

TABLE III

1908 Series. Selective Elimination of "Odd" Locules within the Chief Asymmetry Classes

Radial Asymmetry	Difference in Number of Odd Locules in Eliminated and Matured Ovaries.
.0000	—.335
.4714	—.106
.8165	—.115
.9428	—.454
1.2472	—.157

able to ascertain whether one or both of the characters are of some selective value.

Tables III. and IV. show the results for the 1908 material. The differences are not large,

TABLE IV

1908 Series. Selective Elimination for Radial Asymmetry within the four Locular Composition Types

Character of Ovary.	Difference in Radial Asymmetry in Elimination and Matured Ovaries.
3 "even"	—.017
2 "even" + 1 "odd"	—.019
1 "even" + 2 "odd"	—.034
3 "odd"	—.077

but they are consistent throughout. This indicates, I think, that both characters are of some independent selective value.

Change in Number of Locules per Ovary due to Selective Elimination.—Normally in *Staphylea* the fruit is three-celled, but not infrequently (in some individuals especially) those with two and those with four cells occur.

To determine finally whether either of these types has better chances of surviving than the others would require very large series of observations.

So far as the results from 2,000 eliminated and 3,000 matured ovaries are trustworthy

TABLE V

1908 Series. Showing Elimination of Dimerous Ovaries

Description of Ovaries.	Actual Frequencies.		Percentage Frequencies.	
	Eliminated.	Matured.	Eliminated.	Matured.
2-celled ovaries	54	52	2.48	1.49
3-celled ovaries	2,095	3,355	96.14	96.05
4-celled ovaries	30	86	1.38	2.46
Total ovaries	2,179	3,493	100.00	100.00

Table V. shows that dimerous ovaries are more liable to elimination than tetramerous ones.

Summary.—From the constants in the foregoing sections there can be little doubt concerning the fact of a selective elimination of the ovaries of *Staphylea* during their development from flowering time to the maturing of the fruit. By this selective elimination the mean number of ovules is increased, the mean radial asymmetry is lowered, the proportion of ovaries with odd numbers of ovules in one or more locules is very stringently cut down, and perhaps the mean number of locules per fruit slightly raised.

These results are, I am inclined to think, of considerable importance from the standpoint of morphology and physiology. They show that a physiological unfitness—an incapacity for developing to maturity—is coupled with certain definite morphological characters. Personally I take it that we are not to assume that low numbers of ovules, high radial asymmetry and the presence of "odd" locules are fundamental causes of the incapacity for development, but rather that both morphological and physiological peculiarities are dependent upon some inherent abnormality of the growing point which morphologically finds its expression in the structural features of the fruit and physiologically in its relative capacity for development. These interrelationships between slight aberrations of structure and the capacity of organs for performing their functions offer a most attractive field for research.

In their bearing on the problem of organic evolution the results outlined in this paper are of interest in showing that natural selection may act upon the organs of an individual as well as upon the individual organisms of a population. Without knowing whether the characters we have investigated are inherited it is impossible to say that this elimination is a factor in maintaining the present type of the species.[5] And to argue that this kind of natural selection has been of significance in evolving the considerable degree of radial symmetry found in the fruits of many species of plants with compound ovaries would be stepping too far from a secure pier of facts into the uncertain bog of speculation.

J. Arthur Harris

Cold Spring Harbor, N. Y.,
September 8, 1910

[5] It seems to me unlikely that we shall ever be so fortunate as to find many cases of Darwinian evolution going on in nature. That a constant selection may maintain a type already secured, and that one may be able to observe and measure the intensity of this factor, seems much more probable.